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THE PSYCHOLOGICAL REVIEW

RATING SCALES, DISCRIMINABILITY, AND INFORMATION TRANSMISSION[1]

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Rating scales have been so widely used in psychology, both as research tools and in practical applications, that the determination of the optimum number of rating categories has been an important research problem in itself. The usual attack on this problem has been to determine the effect of number of rating categories on interrater reliability, and research on these lines has been well summarized by Guilford (1954, p. 289 ff.).

The purpose of a rating scale is to allow a rater or group of raters to demonstrate perceptual discrimination of a set of stimuli or objects to be rated. In other words, our concern is not with the rating scale per se, but rather with the extent to which different rating scales allow us to measure discrimination between the stimuli rated. And the basic question about number of rating categories is simply whether for any given rating problem there is an optimum number of rating categories, or at least a number of rating categories beyond which there is no further improvement in discrimination of the rated objects.

Techniques directly concerned with discriminability of stimuli have rarely been used for this problem, and yet since the problem basically is one of stimulus discrimination, it seems most appropriate that they should be.

Two techniques which have been successful in dealing with related problems are the use of information transmission measures and the use of discriminability scaling procedures. Garner and Hake (1951) described how information transmission measures can be used with the absolute judgment problem to determine the effective discrimination between the stimuli of a set. While there are ramifications of the absolute judgment procedure which are different from those of the rating scale situation, the problems are sufficiently alike that we can expect information transmission measures to be useful. Bendig and Hughes (1953) and Bendig (1954) have in fact used the information paradigm for the rating scale problem, and came to the conclusion that discrimination increases with an increase in number of rating categories up to at least 11 categories.

[1] This research was done in part under Contract N5-ori-166, Task Order 1, and in part under Contract Nonr-248(55), between the Office of Naval Research and Johns Hopkins University. This is Report No. 6 under the latter contract. Reproduction in whole or in part is permitted for any purpose of the United States Government. Appreciation is due to Robert Austin, Allen Madans, and Natalia Potanin for aid in various calculations.

In the same paper, Garner and Hake (1951) described a discriminability scaling procedure and pointed out the relatedness of this procedure to the use of information transmission measures. To our knowledge, however, discriminability scaling procedures have not been used in determining the optimum number of rating categories.

The purpose of the present paper is to show how both information measures and discriminability scaling procedures can be used to aid in the solution of the rating scale problem. In addition, it will be shown that these two procedures are quite closely related, and that measures from one can be used to estimate the other. An illustrative set of data will help clarify the nature of these relations.

THE DATA

The data used in these experiments were ratings of the legibility of handwriting samples. We obtained 20 different samples of handwriting of "The quick brown fox jumped over the lazy dogs," chosen to give us a reasonable spread of legibilities. The samples were arranged on a single mimeographed sheet, one to a line. A total of 50 raters then rated each sample on a rating scale with 4, 6, 8, 10, 12, 16, and 20 categories. Each rater rated all samples with each rating scale, using one rating scale until all samples were rated on that scale. Different raters used the rating scales in different counterbalanced orders, and the samples of handwriting were randomized differently on each sheet to minimize any time errors.

The raters were randomly selected from available graduate and undergraduate students. Since each of 50 raters rated each of 20 samples of handwriting on each of seven different rating scales, we had a total of 7,000 ratings as our basic data.

INFORMATION TRANSMISSION

The first analysis was to determine the information transmission, $U(R:S)$, between stimuli, or samples, and responses for each of the rating scales. (While we are using the term information transmission, we will use the more general notation of Garner and McGill, 1956, in which $U(R:S)$ is a contingent uncertainty between responses and stimuli.) All 50 ratings for each sample were pooled for this analysis. Since $U(R:S)$ is limited by the number of response categories, the various rating scales allowed maximum information transmissions from 2.00 bits for four categories to 4.32 bits for 20 categories.

The open squares of Figure 1 are the information transmission measures computed from the basic data, and these data show a consistent rise in information transmission as the number of rating categories is increased. This result is in agreement with the results of Bendig and Hughes (1953) and Bendig (1954). These calculations, however, are subject to a bias in the information measure which changes progressively with number of rating categories.

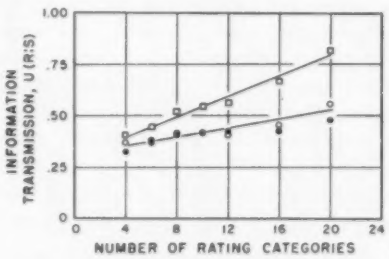

FIG. 1. Relation between number of rating categories and information transmission. (The open squares represent the direct measures of information transmission. The open circles represent the same data corrected for bias by the Miller-Madow correction. The two lines are least-squares fits for these two sets of measures. The closed circles represent estimates of information transmission obtained from discriminability functions.)

Each matrix has the same total number of ratings, but when the number of rating categories is increased, these ratings are distributed more thinly over the larger number of cells in the matrix. Miller (1955) has described a correction for this bias which is known as the Miller-Madow correction, and had himself suggested (Quastler, 1955, fn., p. 16) that Bendig's results would have been different if the correction for bias had been applied.

The correction for bias of information transmission is always in the direction of decreasing the estimate of transmission, and the fewer the number of cases per cell, the greater the correction; i.e., the greater does the directly calculated information transmission exceed the true value. When the Miller-Madow correction is applied, the result is that shown by the open circles of Figure 1. This result, which presents more nearly the true picture, shows considerably less effect of number of rating categories than does the result using raw measures. The correction flattens out the function. However, it is clear that there still is some effect of number of rating categories, and that an increase in the number of categories increases information transmission even up to 20 categories.

The two lines drawn in Figure 1 are linear functions, but they should be considered as entirely empirical description, since logically the functions cannot be linear much beyond the range of conditions investigated. For example, if only one rating category is used, there can be no information transmission, but the functions drawn do not extrapolate to zero with one rating category. Somewhere below four categories they must slope rapidly downwards.

On the other hand, there cannot be a meaningful increase in information transmission if more than 20 rating categories are used. With 20 or more rating categories, each rater can put each stimulus in a different category, and the rater can evidence no more discrimination than to rate each stimulus differently. And clearly the degree of discrimination by the group is limited by the discrimination of each rater. Actually the directly computed information transmission can increase with an increase in number of rating categories until each rater-sample combination produces a different rating. The function corrected for bias, however, would not increase concomitantly, since the bias correction would increase at approximately the same rate as the increase in number of rating categories. In the limit, the information transmission corrected for bias becomes degenerate, since with a large enough number of categories and a small enough number of ratings, it is impossible to demonstrate that the measured information transmission is different from the value expected on the assumption of no true contingency.

DISCRIMINABILITY FUNCTIONS

The second method of determining the amount of discrimination in the ratings is to construct discriminability scales for the set of stimuli. The dispersion of the scale values is then a measure of the relative discrimination between the various stimuli. We have carried out the necessary scaling procedures for these data in two different ways.

Pooled Data

With the first method, the data were kept in the pooled form in which they were used to compute information transmission. The S-R matrix of frequencies was converted into a set of equivalent pair comparisons with the technique described by Guilford (1938). The essence of this procedure is that

the distribution of responses for each stimulus is compared category by category with the distributions for each of the other stimuli to obtain a measure of the equivalent number of times that each stimulus would have been rated greater than each other stimulus, assuming the ratings to be uncorrelated. From the matrix of equivalent pair comparisons, a discriminability scale was then constructed following the usual procedure for pair comparisons. One such discriminability scale was constructed for each rating scale.

Individual Data

With the second method, the ratings were changed to equivalent pair comparisons one rater at a time. Thus each rater's ratings of the 20 stimuli on one rating scale were changed into equivalent pair judgments in much the same way as Guilford (1954, p. 183 ff.) describes for converting ranks. Ties were used where necessary. These individual pair comparisons were then pooled for all 50 raters, and a discriminability scale constructed—again one for each rating scale. We thus have 14 discriminability scales, one for each of seven rating scales with each method.

The purpose in carrying out the two kinds of analysis was to determine whether the pooling of the data for the informational analysis had had any systematic effect on the function relating information transmission to number of rating categories. With a complete multivariate analysis of data such as these, we can determine separately the extent to which responses can be predicted from samples, from raters, and from the interaction between samples and raters. With only one rating per sample per rater, however, we have no choice but to pool the data, and are in effect accepting the interaction term as an error term rather than as a predictability term. We are in effect also accepting the contingent uncertainty between responses and raters as an error term. This last factor, which indicates the extent to which different raters use different responses *on the average*, could change with an increase in number of rating categories. For example, if all raters used rankings when 20 categories were allowed, then there could be no difference in use of responses by different raters, but such differences could exist with smaller members of categories. Thus, although it is not feasible to measure such a term directly, it is possible that the effect of this factor systematically decreased with the increase in number of rating categories, with the resultant increase in the information transmission which was obtained.

The first discriminability scale analysis used the pooled ratings to convert to equivalent pair comparisons. Thus any systematic differences between raters in ratings would still affect the final scale values. With the second scaling procedure, any such differences were removed by changing the data to equivalent pair comparisons before pooling.

Correlations of the Scales

The various scales produced by the two different methods are essentially identical. When the scale values are correlated between the two data processing methods for each of the seven rating scales, the seven correlation coefficients are all above .99. In addition, the correlations between scales for the different rating methods are quite high, as shown in Table 1. We have shown in this table only the correlations for the pooled data. Since all of the correlations between the two scaling methods are so high, a table of correlations using the individual data is practically identical to Table 1.

While the correlations between dis-

TABLE 1

CORRELATIONS BETWEEN DISCRIMINABILITY SCALE VALUES FOR DIFFERENT NUMBERS OF RATING CATEGORIES

	Number of Rating Categories					
	4	6	8	10	12	16
6	.90					
8	.90	.91				
10	.93	.96	.95			
12	.93	.97	.95	.96		
16	.90	.96	.92	.97	.97	
20	.89	.94	.90	.95	.96	.97

Note.—Pooled data were used for these correlations.

criminability scale values obtained from different numbers of rating categories are all quite high, it is clear that rating scales similiar in number of categories correlate more highly than do widely different scales. Thus the 4 scale and the 20 scale have the smallest correlation, while the 16 scale and the 20 scale have the highest correlation, along with some other close pairs.

Discriminability

These data on correlations between discriminability scale values show only that the various scales produced are highly predictable, one from the other. They do not show that the total amount of discrimination is the same. In order to determine the total amount of discrimination, the simplest procedure is to compute the standard deviations of the scale values for each of the scales. The scale whose stimulus values are most spread apart is the scale with the greatest interstimulus discrimination. Consequently, standard deviations have been computed for each of the 14 discriminability scales, and these values are plotted in Figure 2.

These data, like the information transmission data, show an increase in discrimination with an increased number of categories on the rating scale. And while the individual data show slightly greater discrimination than do the pooled data, the difference is very small—mean standard deviation for the pooled data is 0.881 and for the individual data it is 0.897.

What is more important, however, is that there is no evidence that the increase in discrimination with the increase in number of rating categories is in any way due to the pooling of the data. Both sets of points show a definite increase, and only for the 4 category scale is there any appreciable difference between the two standard deviations.

Once again the line drawn is a least-squares solution which can be considered only an empirical description. If one rating category is used, the standard deviation must be zero, and the curve shown does not extrapolate to zero at that point. Likewise, with a larger number of rating categories the scale values could not increase substantially, since no greater precision in equivalent pair comparisons could be obtained if there are more rating categories than there are stimuli. In other words, since the discriminability scale values are obtained from equivalent pair comparisons, the larger range of

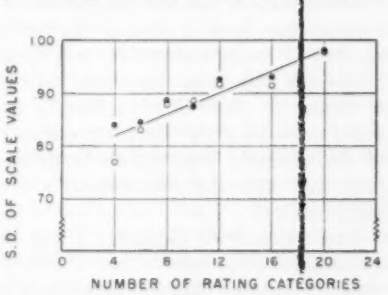

FIG. 2. Relation between number of rating categories and standard deviation of the discriminability scale values. (The open circles are for the data processed as a group; the closed circles for the data tabulated by individuals. The line is a least-squares fit.)

numbers in no way affects the standard deviations of the scale values.

ESTIMATES OF INFORMATION TRANSMISSION

We have shown that two alternative methods of determining the amount of discrimination in a set of stimuli evidenced by a rating procedure lead to the same conclusion, namely, that discrimination increases with an increase in number of rating categories, up to 20 categories in our case. It seems very reasonable that these two measures should, therefore, be related. It is the purpose of this section to show that they are related and that one can be quite exactly estimated from the other.

The actual equation for estimating transmitted information from the standard deviation or variance of the discriminability scale values is

$$\text{est}U(R{:}S) = \tfrac{1}{2} \log_2 [V(\bar{S}) + 1] \quad [1]$$

where $\text{est}U(R{:}S)$ is the estimated information transmission and $V(\bar{S})$ is the variance of the discriminability scale values of the stimuli. The $\bar{S}$ notation emphasizes the fact that these are mean scale values.

For the pooled data shown in Figure 2, an estimate of the information transmission has been computed for each number of rating categories, and these values are shown as the closed circles of Figure 1. It is obvious that Equation 1 provides a very accurate estimate of the unbiased information transmission, and that the two measures are highly related.

The rationale of Equation 1 can be understood from two related points of view. First, we can conceptualize the discriminability scale values as a set of values for a set of stimuli, and this same set of values as the mean numerical responses which would be obtained if both the stimuli and the responses were metricized in the unit normal system used with discriminability scales. In other words, we really have an *S-R* matrix, but one in which these new scale values, rather than the respective original designations, are used for both stimuli and responses. Now with such a matrix both the stimulus scale and the response scale are continuous, but we could compute an information transmission figure from the matrix by categorizing the scales. It is not necessary, however, to set up such a categorized matrix, since uncertainties can be estimated from variances (Garner & McGill, 1956) and we can easily determine the appropriate variances with no need to categorize.

Information transmission can be computed from the following equation:

$$U(R{:}S) = U(R) - U_s(R), \quad [2]$$

where $U(R)$ is the response information or uncertainty, and $U_s(R)$ is the response information conditional on the stimulus, or response equivocation, as it is sometimes called.

$$\text{est}U(R{:}S) = \tfrac{1}{2} \log_2 [V(R)] - \tfrac{1}{2} \log_2 [V_s(R)] \quad [3]$$

In this equation $V(R)$ is the variance of the response continuum, and $V_s(R)$ is the average variance of the responses taking one stimulus at a time, i.e., the within column variance. The two terms on the right side of Equation 3 are the estimates of the equivalent uncertainties of Equation 2. (Actually, each of these terms in Equation 3 should have an associated term which reflects the width of the category interval, but since these terms are the same in both cases, they cancel each other—see Garner & McGill, 1956, for further explanation.)

Now we know that the value of $V_s(R)$ is 1.0, since it is so by definition in a unit normalized system; therefore the logarithm of it is zero.

On the other hand,

$$V(R) = V(\bar{R}) + 1 = V(\bar{S}) + 1. \quad [4]$$

$V(\bar{S})$ is the variance of the actual stimulus scale values, but since we are assuming a one-to-one relation between the responses and the stimuli (except for error variation), the mean response for each stimulus is the same as the stimulus value itself. Thus the variance of the *means* of the responses is the same as the variance of the stimulus scale values. But the total variance of the responses, $V(R)$, will be the variance of the means plus the variance within groups (the error variance in analysis of variance terms). Since the error variance is 1.00, the total variance of the responses in this idealized matrix will be $V(\bar{S}) + 1$.

Thus the estimated second term on the right of Equation 2 is zero, and the first term on the right is directly estimated as shown in Equation 1.

The other point of view from which to understand Equation 1 is by means of correlational analysis. Attneave (1959) has pointed out the general relation that

$$\text{est}U(R{:}S) = -\tfrac{1}{2}\log_2(1 - r^2) \quad [5]$$

Now we can define

$$r_s^2 = V(\bar{S})/[V(\bar{S}) + 1] \quad [6]$$

This correlation coefficient is stated in the usual form as the ratio of the predictable variance to the total variance of a set of scores, and as shown above, the total variance of the responses is $V(\bar{S}) + 1$, and the predictable variance of the responses is $V(\bar{S})$. This correlation coefficient is quite similar to one proposed by Gulliksen and Tukey (1958, p. 108), except that their coefficient is a reliability of *mean* scale values, while the coefficient proposed here is essentially a reliability of individual scores, and is a straightforward variance ratio.

From the relation of Equation 6 ($1\text{-}r_s^2$) is $1/[V(\bar{S}) + 1]$, and from Equation 5, Equation 1 is directly derivable.

Characteristics of the Estimate

This estimating procedure is quite accurate for these data. It should be pointed out, however, that the accuracy of the estimating procedure depends on the normality of the distribution of discriminability scale values, since the estimating equations work accurately, as both Garner and McGill (1956) and Attneave (1959) have pointed out, only when both variances are approximately of the same distribution. This requirement means that the normal distribution is necessary for the mean scale values since the error variance is already forced not only into a unit system but also into a normal system. If the stimulus scale values are off to the extent of providing a rectangular distribution, the error in estimating could be as much as 0.25 bit. The estimating procedure can be used quite generally, however, if a correction factor is estimated for the shape of the distribution.

It is interesting to note that these estimates of information transmission agree very well with the information transmission measures corrected for bias, rather than with the uncorrected measures, even though no unbiasing procedure is inherent in the calculations. The reason for this fact is that the discriminability scales are based on essentially all of the available data, and thus are not themselves subject to the same sorts of bias which operate with the directly computed measures of information transmission. In this respect, the estimating procedure accomplishes much the same effect in minimizing the problem of bias as do the procedures suggested by Attneave

(1955) for pooling data in various ways which are appropriate to specific situations.

THE OPTIMUM NUMBER OF CATEGORIES

The primary purpose of this paper is to demonstrate that information measures and discriminability scales are useful in determining the optimum number of rating categories, and furthermore to show that these two measures are rationally related. It is nevertheless appropriate to discuss the nature of the factors which affect the optimum number of rating categories.

The optimum number of rating categories, or at least the number beyond which there will be no further improvement in discrimination, is clearly a function of the amount of discriminability inherent in the stimuli being rated. Thus there can be no single number of rating categories appropriate to all rating situations. At the limits of discriminability, this fact is obvious. For example, if ratings are made of a set of objects between which there are no perceptible differences, then one rating category will do as well as two or more. If there is no perceptual discrimination, there is no need of a discriminating rating scale.

On the other hand, suppose that we have twenty different objects all of which are clearly discriminable. Then we must have as many rating categories as there are objects in order for raters to demonstrate their ability to discriminate, and ranking will be the most effective procedure. In information terms, we cannot allow the response information to limit the measurable information transmission.

The limiting cases are manifest; the intermediate cases are much more ambiguous. Bendig and Hughes (1953) had their subjects rate 12 countries for familiarity, and when the Miller-Madow correction for bias is applied to their data, there is no improvement in information transmission beyond three rating categories, with which the information transmission obtained is approximately 0.25 bit. When Bendig (1954) had subjects rate preference for 20 foods, increased information transmission (again corrected for bias) was obtained up to at least nine categories, with which 0.32 bit of transmitted information was obtained. In the present experiment, increased discrimination is obtained with up to 20 categories, with which approximately 0.50 bit of transmitted information was measured.

What seems to be clear from these few cases is that the number of useful categories increases with an increase in the amount of information transmission, and that the number of categories needed is far greater than the information transmission figure seems to indicate.

This fact that the number of useful rating categories is far greater than the information transmission seems to indicate is at first glance very surprising, particularly since in the absolute judgment situation, which is so similar, quite another conclusion has been drawn. When information transmission has been measured in the absolute judgment context, the obtained measure has been used to determine the minimum number of categories with which the given information transmission can be obtained, and many experiments have demonstrated the validity of this interpretation of information measures. In other words, it has been demonstrated that there is a channel capacity, such that no further information transmission can be obtained if the number of categories is increased beyond that indicated by the channel capacity.

The absolute judgment paradigm and the rating scale paradigm are in fact,

however, quite different, and the essence of the difference is this: In the absolute judgment paradigm, the number of stimuli and the number of responses are varied concomitantly, while by the nature of the problem they cannot be in the usual rating scale application. In the absolute judgment case, if we have an information transmission of 2.0 bits, we can interpret this figure as indicating that we can obtain just as good discrimination if we use four stimuli and four responses as if we use any larger number. Furthermore, we can and should select the particular four stimuli in such a way that maximum discrimination will be realized.

The nature of the rating scale problem, however, is that we have a fixed number of objects or samples to rate, and we are not free to make this number smaller. If 20 different foods, or 50 men in a plant, are to be rated, we are not really interested in seeing how well we can transmit information if fewer foods or fewer men are rated: we want information about each and every stimulus to be rated.

As long as we are not free to select a small representative set of stimuli (i.e., cannot recode into a more efficient set), then we will need to have a large number of rating categories to obtain relatively small amounts of information transmission. If two adjacent stimuli on a discriminability scale are reliably different, then the rating scale must be sufficiently detailed to allow that difference to be shown.

The nature of this problem is easily shown with reference to the hypothetical set of data shown in Table 2. For this table, the contingent uncertainty, or transmitted information, is 1.07 bits. Now we can easily select two stimuli and two responses which will allow us to obtain 1.00 bit of contingent uncertainty—we simply take the first and fourth stimuli and assign one response to each and the pattern of the data makes it clear that we can expect to obtain no confusions between these stimuli. Thus we will obtain 1.00 bit of transmitted information.

TABLE 2

AN ILLUSTRATIVE CONTINGENCY TABLE

Responses	Stimuli				
	1	2	3	4	Σ
1	0.21	0.04			0.25
2	0.04	0.17	0.04		0.25
3		0.04	0.17	0.04	0.25
4			0.04	0.21	0.25
Σ	0.25	0.25	0.25	0.25	1.00

Note.—Cell entries are proportions of joint occurrence.

Suppose, however, that we must keep all the stimuli, but want to reduce to two response categories. The best we can do is to group Response 1 with 2 and also 3 with 4. But since the responses to the second and third stimuli will overlap these two new response categories, there must remain some response equivocation, and we cannot obtain the maximum of 1.00 bit of information transmission. In this actual case, if the data from Table 2 are pooled to form the 4×2 matrix, the resultant information transmission is 0.68 bit.

Thus the essential difference between the absolute judgment situation and the rating scale situation is not in the character of the stimuli, but rather in the fact that we feel free to select stimuli in one case and do not in the other. If loudness ratings were obtained under equivalent conditions, i.e., if we keep a fixed and large total number of stimuli, but change the number of rating categories, information transmission would vary with the number of rating categories also.

The small amount of available data suggest that the response information

(i.e., logarithm to the Base 2 of the number of categories) should be about 5 to 10 times as large as the expected information transmission, at least for small amounts of information transmission. For small numbers of stimuli to be rated, it is almost certainly worth using complete ranking rather than coarser scales. And as a last statement, it is clear that information transmission cannot be lost by increasing the number of rating categories. Therefore, it is better to err on the side of having too many categories than to err by having too few.

SUMMARY

The purpose of this paper is to show how information transmission measures and discriminability scales can be used to determine the optimum number of rating categories. An illustrative set of data were obtained for 20 samples of handwriting with 4, 6, 8, 10, 12, 16, and 20 rating categories. Information transmission measures, after correction for sampling bias, show a small but definite increase in discrimination up to the 20 categories used. Discriminability scale values were also constructed from the rating data, and the standard deviations of the scale values show the same increase in discrimination. The variance of the discriminability scale values can be used to estimate information transmission, and the estimates agree very well with the directly calculated information measures. It is pointed out that the optimum number of rating categories must depend on the discriminability inherent in the particular rated stimuli, and that the optimum number of categories will be large compared to the measured information transmission.

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THE MULTIPLE ABSTRACT VARIANCE ANALYSIS EQUATIONS AND SOLUTIONS:

FOR NATURE–NURTURE RESEARCH ON CONTINUOUS VARIABLES

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Essential Nature of the Method

Research in psychological genetics operates either on: (*a*) particulate behavior syndromes, e.g., Huntington's chorea, phenylpyruvic amentia, which are visibly either present or absent, and which can usually be directly handled by Mendelian concepts and analyses applied to familial observations; or, (*b*) apparently continuously distributed behavior traits, the variance on which one can attempt to assign to various environmental and genetic sources. Some inferences in terms of the former can to a slight degree be made from the latter type of biometrical and population genetics observations through the mathematical treatments of Fisher (1930), Haldane (1932), Kempthorne (1957), Mather (1949), Wright (1931), and others. However, psychologists would in general be content, and, indeed, greatly heartened, even to be given some reliable information about the nature–nurture variance ratios for the more important continuous human personality and ability dimensions. For the overwhelming majority of psychological measures are, as far as we can see, continuous, and the instances that can be directly analyzed by particulate Mendelian analysis are negligibly few.

Until a few years ago all published instances of nature–nurture variance analysis ratios for psychological variables were based on studies of identical and fraternal twins (sometimes sibs). At that time the present writer (1953) put forward a design called the Multiple Abstract Variance Analysis method, which seemed capable of answering more questions than the twin method and of avoiding certain artificialities and limitations therein. Although developed independently in the field of psychological measurements, it proved on examination to be much the same in general principle as the estimation of components of variance used in agricultural and biological genetics. However, the MAVA method (as we shall henceforth abbreviate it) contains features unique to the human cultural situation and to psychological measurement. Moreover, it is new to psychologists, being exemplified in only two studies. The first, measuring personality factors by questionnaire, occurred in 1955 (Cattell, Blewett, & Beloff), and the second, using objective, behavioral batteries for the personality factors, and a culture-fair measure of intelligence, was published in 1957 (Cattell, Kristy, & Stice).

[1] The writer wishes to express his great indebtedness for discussion and advice to the following (who are nevertheless not to be held responsible for conclusions): geneticists N. E. Morton and W. J. Schull; geneticists and statisticians H. W. Norton and R. W. Touchberry of the University of Illinois; and statisticians D. L. Burkholder, R. A. Fisher, Q. McNemar, and, especially, C. R. Rao of Calcutta. He wishes further to thank his assistants Merlin Foster, John Horn, and Marcia Sedgwick for their persistence and care in the algebraic treatment of the equations.

The results of these first applications, in that they demonstrated diverse and characteristic nature–nurture ratios for well known personality dimensions, agreeing, moreover, with ulterior evidence, were encouraging. It seemed very desirable, therefore, to develop further the potentialities of the method itself.[2] This paper sets out to make a closer examination of the method, its assumptions, its calculations, and its needs in terms of more refined development.

The title of the method derives from the fact that *conceptual* variances are introduced, i.e., variances which can only be inferred or "abstracted" from actual, observed variances. The label designates this abstractness in order to avoid confusion with the usual use of the analysis of variance design in which questions are asked purely in regard to concrete variances. It is "multiple" because it views each concrete observed variance as the result of several, abstract, inferred variances. The MAVA method in fact postulates that the observed variance on a given trait, e.g., a personality factor score, measurable on a continuum, can be resolved into four contributory variances as follows:

Variance through causes resident in the differences of environment *between* families. Henceforth written σ^2_{be}.

Variance through causes resident in the differences of heredity *between* families. Henceforth written σ^2_{bh}.

Variance through causes resident in the differences of environment for individuals *within* the family. Henceforth, σ^2_{we}.

Variance through causes resident in the differences of heredity between siblings *within* the same family (gene segregation). Henceforth, σ^2_{wh}.

Furthermore, it does not assume that these sources are uncorrelated but attempts simultaneously to determine what correlations exist between these hereditary and environmental variances, both within families (henceforth, $r_{we.wh}$), between families (henceforth, $r_{be.bh}$), and, occasionally, through the rare correlation of a within and between variance. Obviously, the equations below imply that heredity and environment act mutually in a simple additive fashion, *without* any "catch all" category to accommodate to all kinds of interaction effects, as usually conceived in analysis of variance. At the present stage of the model, one could either assume heredity and environment to be uncorrelated and then permit interaction, as in analysis of variance, or sacrifice interaction. The assumption of uncorrelated heredity and environment seemed to us, in the psychological field, less tenable.[3]

A more ideal model would be one which both allows covariance, as here, and also interaction, but, while such a model is being worked out, the present model can be expected to give the usual approximate fit of a simpler model to a, possibly existent, more

[2] The writer has to apologize for five years of delay in presenting this fuller statement, occasioned by nothing more than writing difficulties in presenting a simultaneously adequate explanation of genetic concepts, for psychological readers, and of psychological measurement assumptions, for geneticists, in anything less than book length. The delay is regrettable because psychological genetic researches have been started in the interim on what is essentially an obsolete twin design, or researchers have been compelled laboriously to derive independently from the initial statement (1953) the formula now given in Tables 1 and 2. This situation may perhaps justify the present necessarily uncomfortably condensed presentation.

[3] Were it not that one sometimes hears the remarkable statement that "psychological traits cannot be wholly resolved into genetic and environmental causes" it might be unnecessary to add here that we assume that the variances and covariances are *exhaustive* of the measured trait variances.

complex natural pattern. Additionally, the assumption will be recognized that the values obtained for the variances and correlations are specifically those existing in *the given cultural and racial sample* (referable, if desired, to parameters for a statistical population of that type), and that generalization beyond this, e.g., to modern, primitive, etc. cultures, is possible only in the light of additional, strategic, experiments. Since a complete statement of assumptions in a model can be endless, the reader should accept those obviously implied, while others will be mentioned later as they become relevant.

The general manner of building equations relating observed to abstract variances will be evident from inspecting, say, Equation 4 in Table 1 below. Here it is stated that the observed variance within a series of pairs of siblings reared together, σ^2_{ST}, is the sum of within family environmental variance, σ^2_{we}, plus within family heredity variance (due to different segregations of parental genes), σ^2_{wh}, plus the covariance (if these influences are correlated), $2r\sigma_{we}\sigma_{wh}$. The calculation of the *observed* variances, as such, will be discussed later. Originally only six such equations were written (Cattell, 1953), because it was presumed that there would only be six unknowns—four variances and two correlation coefficients. Thus by the solution of six simultaneous equations, using the six experimentally determinable concrete variances—σ^2_{ST}, σ^2_{SA}, etc.—it was hoped to solve for the six abstract, conceptual values.

However, six equations and six observed variances are not enough, because: (*a*) The last two or three of the six equations import other unknowns. For example, one of our necessary equations was for twins reared together and, unless one makes the psychologically unsatisfying assumption that the typical environmental difference in the lives of twins is the same as that between sibs, a new term, for a special kind of intersib environmental difference, $\sigma^2_{we''}$, must be introduced. (*b*) Similarly it turns out that with additional experimental groups more than the above *two* heredity–environment covariances are involved. (*c*) Linear dependencies systematically appear between the equations for "within," "between," and "total" variances in each of the family types one uses, thus reducing the number of effective equations.

The additional equations that may be used, and the alternative assumptions possible in using various combinations will be discussed *after* the following section, clarifying the calculation of concrete variances.

CALCULATIONS AND ASSUMPTIONS CONCERNING CONCRETE, EXPERIMENTALLY MEASURED VARIANCES

Conceptually, the within family variance, on some such trait as intelligence, surgency, etc., is the variance (*a*) among the children, i.e., not including the parents; (*b*) of the typical American family (or whatever cultural or ethnic group one studies); (*c*) affected by size of family only statistically, i.e., to the extent that the variance estimate uses different degrees of freedom with different family sizes (actually we took mostly *pairs* but occasionally trios, from which to estimate this variance); (*d*) corrected for age differences, i.e., it is the variance that would exist, on traits susceptible to age change, if all sibs were brought to the same age; (*e*) corrected for experimental error of measurement, but *not* for function fluctuation.

The justification for using the con-

cept thus is partly experimental practicability and partly the achievement of maximum negotiability in further theoretical or calculation use of the obtained variances. Objection can be raised to (*c*) that large families might have, for example, smaller variance, on certain traits, due to more powerful group "coercion to the bio-social mean" (Cattell, 1957). This can be investigated by our design, through an experiment with a sample of small size families contrasted with one of large size. Meanwhile the adopted assumption is simpler. As to (*d*), it is true that by allowing an age correction we are already here using an "abstracted" rather than a literal, concrete variance; but there is no point in confounding psychological variance with age variance. If one knows the age variance of families in a given culture, the concrete variances can always be readily "restored" from the obtained variances through this more analytical approach and, meanwhile, the nature–nurture ratio is more pure, comparable, and comprehensible for an age-constant group.

The issue in (*e*) is best seen by setting out the equation:

$$\underset{\substack{\text{True (Individual Difference)} \\ \text{Variance}}}{\sigma^2_t} = \underset{\substack{\text{Observed} \\ \text{Variance}}}{\sigma^2_o} - \underset{\substack{\text{Error of} \\ \text{Measurement}}}{\sigma^2_e} - \underset{\substack{\text{Function} \\ \text{Fluctuation}}}{\sigma^2_f} \qquad [1]$$

Although the allowance for error of measurement, derived from the *dependability* coefficient of the test (Cattell, 1957) by $\sigma^2_e = 1 - r_{rd}$, is always only an estimate, it is better than not allowing for it at all. The question whether function fluctuation should be taken out depends on one's conception of environment. Periodic or aperiodic, short-interval, function fluctuation occurs in most personality and ability measures (Cattell, 1957; Fiske & Rice, 1955; Thouless, 1936) from internal or external causes. Should one say that the ergs of hunger or sleep or the anxiety reaction are not innate because they fluctuate hourly? The writer is inclined to the view that over a sufficient series of standard situations a reasonably stable strength level could be assigned to an individual on such fluctuant traits and that it is *this* level which we speak about when we talk of individual differences, e.g., in anxiety. For most purposes it is the nature–nurture contribution to this relatively stable "over a couple of months" value, at various age levels, that we are interested in. Function fluctuation variance is calculable from the difference of the dependability and stability coefficients (Cattell, 1957), but, for lack of the latter it was not actually taken out of the variance in our experiments (Cattell, 1957; Cattell, Blewett, & Beloff, 1955). When not taken out it theoretically swells the environmental variance, and it is significant that the distribution of ratios for two dozen personality dimensions in our studies was decidedly skewed to the environmental predominance end. If experimental error is not taken out, it also predominantly swells the variance ascribed to environment. Ideally, therefore, both should be taken out before calculating the nature–nurture ratios.

The assumptions and problems in the estimation of the observed between-family variance are essentially of the same nature. Again, one is estimating a value from the sample for a statistical population assumed to be homogeneous with the sample, and, in this case, the correctness of (*b*) above—that the statistical population

is the typical American population—is harder to justify in the circumstances commonly prevailing in nature–nurture studies. For, without quite extraordinary exertions by the experimenters, a disproportionate number of cases will come from social welfare agencies. Obviously, the ideal requirement is a k-way classification of families by religion, occupation, age of parents, economic, social, education, etc., background of parents, etc., corresponding to a stratified population sample (or, when investigating the effect of particular conditions, e.g., religion, on the ratios, a deliberately selected sample).

Another specification which must be watched in defining and using the various variance ratios from the MAVA is whether the differences considered are between boy, girl,[4] or boy-and-girl families. Even in the third case the equation for identical twins must, naturally, remain that for same sex pairs; but, provided $\sigma^2_{we''}$ has been used for the special between-twin environmental variance these same sex pairs can be used in the total solution.

As to the degrees of freedom, the *within-family* values, when pairs of children are taken (deviation of each from their common mean, squared and added over k families), will be k. When n children instead of two are taken from each family it will be $k(n-1)$. For *between-family* variance, obtained by subtracting the grand mean of all (sib) families from the mean of each family, squaring and adding, it will be $k-1$.

CONCEPTS AND ASSUMPTIONS IN THE EQUATIONS

It will be evident from the initial illustration by the case of within-family environmental variance above that the experimenter in this area is placed in a theoretical dilemma peculiarly embarrassing to a perfectionist. As he adds more equations in the hope of more completely determining unknowns, he adds new unknowns. In the writer's opinion one can achieve a complete solution in the sense that the number of experimental situations capable of yielding new equations eventually overhauls the number of unknowns. But since it is not beyond the wit of man to invent even finer conceptual subdivisions of family types and situations to which he would like a separate answer, let us admit that the process is asymptotic. Consequently, it is up to the experimenter to draw a line where he will in data gathering, in the light of present estimations of magnitudes of various influences and the steps dictated by research strategy.

Accordingly, the presentation is cut to two designs, one, called the Limited Resources Design, which makes grosser assumptions in order to accommodate to the resources of nine out of ten investigators, and the other, called the Complete Design, which aims at what, by ordinary standards, can be called a complete solution, regardless of difficulty or expense. The assumptions in these two designs can best be discussed when we have considered the symbols used, as follows:

Abstract Variances

1. σ^2_{we} = trait variance (between sibs) due to within-family environmental (*we*) variation. Incidentally, we assume that this variability of environment among children raised in the same family is essentially the same for all kinds of families, including normal and adoptive families. On the other hand in the special case of fraternal twins, who are of identical age, and identical twins, who, according to clinicians, have an urge to be more

[4] Not only environmental, but also hereditary within family variances could be different for girl-girl pairs, due to the extra chromosome.

alike or (in other opinions) more individualistic, we enter with special terms capable of discovering such differences, as follows:

2. $\sigma^2_{we'}$ = trait variance within family associated with type of environmental difference peculiar to fraternal twins.
3. $\sigma^2_{we''}$ = trait variance within family associated with type of environmental difference peculiar to identical twins.
4. σ^2_{wh} = trait variance due to within-family heredity (*wh*) variation.
5. σ^2_{be} = trait variance due to between-family environment (*be*) variation.
6. σ^2_{bh} = trait variance due to between-family hereditary (*bh*) variation.

Another possibility—that the within-family environmental variance among sibs might be different in natural and adoptive families—has not been given separate representation at this point.

Correlations

7. $r_{wh\cdot we}$ = correlation between effects of within family heredity and within family environment (siblings).

A systematically different value for this correlation might arise in special circumstances, notably (*a*) when the two sibs are of the same age, i.e., fraternal twins. For example, a trait which might be fostered in a younger by an older brother might be inhibited in the interests of competition when they are the same age; (*b*) when it is an adoptive family, i.e., not true sibs and raised by foster parents. For, whether we believe with G. B. Shaw that parents are allergic to their own hereditary defects appearing in their children, or take a more kindly view that they will be more understanding, the $r_{we\cdot wh}$ will at least be different. Moreover, the hereditary variance in adoptive (e.g., foster) families is typically greater than for ordinary families, being a sum (or other combination) of within and between hereditary variances. If there are reasons for r settling down to a certain value with a certain variance, it is unlikely to remain at the same value when the range of one variable is altered. Actually we have assumed here that increased variance of heredity (Equation 6, Table 1) and of environment (Equation 5, Table 1) affect the $r_{wA\cdot we}$ term in the same essential fashion and we have represented both by a single new r as follows:

8. $r'_{wh\cdot we}$ = correlation of heredity and environment in adoptive families.

In such families also, it will be noted, we have the peculiar condition of a possible correlation between a between-group and a within-group variance, in that the foster parents may, for example, treat the children differently according to the hereditary variations appearing from the families from which they derive. Hence:

9. $r'_{we\cdot bh}$ = correlation of within-environment and between-heredity peculiar to adoptive families. A prime is placed on this r, though there is no other $r_{we\cdot bh}$ from which to distinguish it, because it helps classification to have all r's peculiar to adoptive families marked with a prime.
10. $r_{bh\cdot be}$ = correlation between effects of between-family heredity and between-family environment. The frequently discussed correlation of "good" heredity with "good" environment among families is a possible instance.

There follow two possible correlations, probably nonzero only because of what may be called "placement agency" effects. In our society this arises partly from the deliberate policy of welfare agencies of placing more able children in families likely to give them a better educational environment, but in most societies it might arise from a tendency to adopt orphaned *relatives*. These placement agency r's are marked by a double prime, as follows:

11. $r''_{wh\cdot be}$ = placement agency correlation peculiar to sibs raised in different families, where within-family hereditary differences may become correlated with between-family environment by deliberate intention of social agencies in placing, e.g., brighter sib in more educated family.
12. $r''_{bh\cdot be}$ = placement agency correlation, through any systematic tendency to match biological family characters of adopted children to environmental characters of foster parent families.

Between four main variables—*we*, *wh*, *be*, *bh*—there are six possible correlations (more, with special variables above) of which four would normally be dropped on the assumption that no correlation commonly exists of within and between variances. However, having found two exceptions we should look for more. The possibility that $r_{wh.bh}$ and $r_{we.be}$ could be nonzero is however, hard to conceive. For example, it is difficult to see any way in which an individual's deviation in heredity from his family mean could be connected with the fact that his family differs in mean from the community hereditary mean. On the other hand, if, say, specific "roles" are more emphasized in highly than poorly educated families, the possibility exists that an individual's deviation environmentally from his family mean will be greater when his family is environmentally most positively deviant from the community mean. This would give a significant curvilinear correlation but no significant linear correlation. This will be recognized as another restriction on our model, that it does not respond to curvilinear correlation among variances. But it does mean that no especial reason exists for complicating our equations with an $r_{be.we}$ term. We are left, therefore, with two "main" (r) correlations, two "component" (r') correlations, and two "placement" (r'') correlations requiring terms for solutions.

If space permitted, a number of other interesting theoretical possibilities could be discussed here, though most would be rejected as highly improbable. What may seem an undue refinement of conception, but is nevertheless vital in later interpretations and applications, is implicit in the above labeling of within and between-family hereditary and environmental variances as "effects" not "influences." It would not be correct, for example, rigidly to reason that all environmental variance within families comes from influences within the family or all environmental variance between families from influences within the community operating differently on these families. Similarly the correlation of a within-family hereditary deviation with a within-family environmental effect does not imply that the correlation is brought about entirely by influences within the family. Tracing influences is a second step after locating effects.

To determine the 12 unknowns above (6 variances, 6 r's), which we have found it necessary to introduce on theoretical grounds in the Complete Design, one must employ, for reasons stated, more than 12 equations. The following 16 experimentally determinable variances, each for a particular kind of group, are required to represent the left side of each of the equations needed. (The equations themselves are set out in Tables 1 and 2).

1. σ^2_{ITT} = variance in the trait as measured among identical twins reared together by their parents.
2. σ^2_{ITA} = variance in the trait as measured among identical twins reared apart.
3. σ^2_{FTT} = variance in the trait as measured among fraternal twins

raised together by their true parents.

4. σ^2_{ST} = variance in the trait as measured among ordinary siblings reared together by their parents.
5. σ^2_{SA} = variance in the trait as measured among ordinary siblings reared apart.
6. σ^2_{UT} = variance in the trait as measured among unrelated children reared together in one family with foster parents who are biological parents of none.
7. σ^2_{UA} = variance in the trait as measured among unrelated reared apart (general population).
8. σ^2_{BITTF} = variance in the trait as measured between the means of identical twin pairs in their true families.
9. σ^2_{BITAF} = variance in the trait as measured between the means of identical twin pairs raised apart.
10. σ^2_{BNF} = variance in the trait as measured between the means of natural families, i.e., means of sib pairs (or trios, etc.) reared together.
11. σ^2_{BBF} = variance in the trait as measured between the means of biological families whose siblings are reared in different foster homes.
12. σ^2_{BSF} = variance in the trait as measured between the means of social families, i.e., adoptive families of biologically unrelated children.
13. σ^2_{HST} = variance in the trait as measured among half-siblings raised together.
14. σ^2_{HSA} = variance of half-siblings raised apart.
15. σ^2_{BHSTF}[1] = variance in the trait as measured among means between half-siblings raised together by one true parent.
16. σ^2_{BHSAF} = variance of means between half-siblings raised apart.

Other familial groups yielding experimentally determinable variances are theoretically conceivable, but they demand too much tact on the part of the experimenter in locating them, or too much moral aberration on the part of the community, to be practicable!

[1] A further specialization can be made here according to whether child is raised with mother or father.

EQUATIONS FOR THE COMPLETE SOLUTION AND THE LIMITED RESOURCES DESIGNS

The 16 equations which link the above experimental, concrete variances with the abstract variances and correlations seemed to produce them are shown in Table 1 below. We assume they will require no discussion, beyond the illustration already given of a typical equation (Equation 4) for most readers. Perhaps a few words on a more esoteric example —Equation 13, half-sibs reared together—will complete the explanation.

Here we might have two children of one mother by two fathers. Incidentally, introduction of a different term for children of two mothers by one father is not considered necessary at this stage, nor is the variance considered modified by the different heads of family in which the two half-sibs might be brought up together. They will differ by (*a*) within-family heredity variance, σ^2_{wh}. For each is only one "choice" among the possible parental gene segregations; (*b*) also, in this case, by half the interfamily hereditary variance, $\frac{1}{2}\sigma^2_{bh}$, for one parent is the same and one different; and (*c*) by the usual within-family environmental differences, σ^2_{we}. In terms of covariances there will be (*d*) a "component variance" correlation of the between-family hereditary variance, (which is now part of the total within-family environmental variance) with environmental variance in the family. This produces the covariance term, $\sqrt{2}r'_{we.bh}\sigma_{we}\sigma_{bh}$. (The coefficient $\sqrt{2}$ before this is derived from $2/\sqrt{2}$, the usual coefficient

TABLE 1

THE SEVENTEEN GENERALLY PRACTICABLE EQUATIONS FOR THE COMPLETE MULTIPLE VARIANCE DESIGN

Note.—The arrangement of equations in Table 1 can, for purposes of simplification be regarded as creating two blocks: 1. 1 through 12, which are constellations of ordinary sib and twin families, and which divide into *within* variances (1 through 6) and *between* variances (8 through 12), and, 2. constellations in half-sib families, again the first half being *within* and the second *between* variances. The matching of each *between* with its corresponding *within* is indicated by (Suppl. "X")—Supplementary to the earlier numbered equation—written beside the matched equation.

$\sigma^2_{ITT} = \sigma^2_{we''}$ [1]

$\sigma^2_{ITA} = \sigma^2_{w} + \sigma^2_{be}$ [2]

$\sigma^2_{FTT} = \sigma^2_{wh} + \sigma^2_{we'} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we'}$ (or replace last term by $2r_{wh\cdot we'}\sigma_{wh}\sigma_{we'}$) [3]

$\sigma^2_{ST} = \sigma^2_{wh} + \sigma^2_{we} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we}$ [4]

$\sigma^2_{SA} = \sigma^2_{wh} + \sigma^2_{we} + \sigma^2_{be} + 2r'_{wh\cdot we}\sigma_{wh}\sigma_{we}[+ 2r''_{wh\cdot be}\sigma_{wh}\sigma_{be}]$ [5]

$\sigma^2_{UT} = \sigma^2_{wh} + \sigma^2_{we} + \sigma^2_{bh} + 2r'_{wh\cdot we}\sigma_{wh}\sigma_{we} + 2r'_{we\cdot bh}\sigma_{we}\sigma_{bh}$ [6]

$\sigma^2_{UA} = \sigma^2_{wh} + \sigma^2_{we} + \sigma^2_{bh} + \sigma^2_{be} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we} + 2r_{bh\cdot be}\sigma_{bh}\sigma_{be}$ [7]

(Suppl. 1)

$\sigma^2_{BITTF} = 2\sigma^2_{wh} + \sigma^2_{we} + 2\sigma^2_{bh} + 2\sigma^2_{be} + 2\sqrt{2}r_{wh\cdot we}\sigma_{wh}\sigma_{we} + 4r_{bh\cdot be}\sigma_{bh}\sigma_{be}$ [8]

(Suppl. 2)

$\sigma^2_{BITAF} = 2\sigma^2_{wh} + \sigma^2_{we} + 2\sigma^2_{bh} + \sigma^2_{be} + 2\sqrt{2}r'_{wh\cdot we}\sigma_{wh}\sigma_{we} + 2\sqrt{2}r''_{bh\cdot be}\sigma_{bh}\sigma_{be} + 2\sqrt{2}r'_{we\cdot bh}\sigma_{we}\sigma_{bh}[+ 2\sqrt{2}r''_{wh\cdot be}\sigma_{wh}\sigma_{be}]$ [9]

$[\sigma^2_{BFTT} = \sigma^2_{wh} + \sigma^2_{we'''} + 2\sigma^2_{bh} + 2\sigma^2_{be} + 2r_{wh\cdot we'''}\sigma_{wh}\sigma_{we'''} + 4r_{bh\cdot be}\sigma_{bh}\sigma_{be}]$ [9a]

(Suppl. 4 or 3 and 4)

$\sigma^2_{BNF} = \sigma^2_{wh} + \sigma^2_{we} + 2\sigma^2_{bh} + 2\sigma^2_{be} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we} + 4r_{bh\cdot be}\sigma_{bh}\sigma_{be}$ [10]

(Suppl. 5)

$\sigma^2_{BBF} = \sigma^2_{wh} + \sigma^2_{we} + 2\sigma^2_{bh} + \sigma^2_{be} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we} + 2\sqrt{2}r'_{we\cdot bh}\sigma_{we}\sigma_{bh} + 2\sqrt{2}r''_{bh\cdot be}\sigma_{bh}\sigma_{be}[+ 2r''_{wh\cdot be}\sigma_{wh}\sigma_{be}]$ [11]

(Suppl. 6)

$\sigma^2_{BSF} = \sigma^2_{wh} + \sigma^2_{we} + \sigma^2_{bh} + 2\sigma^2_{be} + 2r'_{wh\cdot we}\sigma_{wh}\sigma_{we} + 2r'_{we\cdot bh}\sigma_{we}\sigma_{bh} + 2\sqrt{2}r''_{bh\cdot be}\sigma_{bh}\sigma_{be}[+ 2\sqrt{2}r''_{wh\cdot be}\sigma_{wh}\sigma_{be}]$ [12]

$\sigma^2_{HST} = \sigma^2_{wh} + \sigma^2_{we} + \frac{1}{2}\sigma^2_{bh} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we} + \sqrt{2}r'_{we\cdot bh}\sigma_{we}\sigma_{bh}$ [13]

$\sigma^2_{HSA} = \sigma^2_{wh} + \sigma^2_{we} + \frac{1}{2}\sigma^2_{bh} + \sigma^2_{be} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we} + \sqrt{2}r_{bh\cdot be}\sigma_{bh}\sigma_{be}$ [14]

(Suppl. 13)

$\sigma^2_{BHSTF} = \sigma^2_{wh} + \sigma^2_{we} + \frac{3}{2}\sigma^2_{bh} + 2\sigma^2_{be} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we} + 2\sqrt{3}r_{bh\cdot be}\sigma_{bh}\sigma_{be}$ [15]

(Suppl. 14)

$\sigma^2_{BHSAF} = \sigma^2_{wh} + \sigma^2_{we} + \frac{3}{2}\sigma^2_{bh} + \sigma^2_{be} + 2r_{wh\cdot we}\sigma_{wh}\sigma_{we} + \sqrt{6}r_{bh\cdot be}\sigma_{bh}\sigma_{be}$ [16]

2 being multiplied by the root of a half because σ^2_{bh} is present only as a *half* variance.) (*e*) The usual covariance of heredity and environment, $2r_{we.wh}\sigma_{we}\sigma_{wh}$. Other equations are similarly derived and from consideration of each it will be evident that a good deal of careful reasoning from first principles, and discussion among experts, has gone into them.

TABLE 2

THE TEN EQUATIONS USED IN THE LIMITED RESOURCES DESIGN

$$\sigma^2_{ITT} = \sigma^2_{ww'} \quad [1]$$

$$\sigma^2_{FTT} = \sigma^2_{wh} + \sigma^2_{ww'} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww'} \quad [2]$$

$$\sigma^2_{ST} = \sigma^2_{wh} + \sigma^2_{ww} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} \quad [3]$$

$$\sigma^2_{SA} = \sigma^2_{wh} + \sigma^2_{ww} + \sigma^2_{bw} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} \quad [4]$$

$$\sigma^2_{UT} = \sigma^2_{wh} + \sigma^2_{ww} + \sigma^2_{bh} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} + 2r'_{ww\cdot bh}\sigma_{ww}\sigma_{bh} \quad [5]$$

$$\sigma^2_{UA} = \sigma^2_{wh} + \sigma^2_{ww} + \sigma^2_{bh} + \sigma^2_{bw} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} + 2r_{bh\cdot bw}\sigma_{bh}\sigma_{bw} \quad [6]$$

$$\sigma^2_{BITTF} = 2\sigma^2_{wh} + \sigma^2_{ww} + 2\sigma^2_{bh} + 2\sigma^2_{bw} + 2\sqrt{2}r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} + 4r_{bh\cdot bw}\sigma_{bh}\sigma_{bw} \quad [7]$$

$$\sigma^2_{BNF} = \sigma^2_{wh} + \sigma^2_{ww} + 2\sigma^2_{bh} + 2\sigma^2_{bw} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} + 4r_{bh\cdot bw}\sigma_{bh}\sigma_{bw} \quad [8]$$

$$\sigma^2_{BBF} = \sigma^2_{wh} + \sigma^2_{ww} + 2\sigma^2_{bh} + \sigma^2_{bw} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} + 2\sqrt{2}r'_{ww\cdot bh}\sigma_{ww}\sigma_{bh} + 2\sqrt{2}r''_{bh\cdot bw}\sigma_{bh}\sigma_{bw} \quad [9]$$

$$\sigma^2_{BSF} = \sigma^2_{wh} + \sigma^2_{ww} + \sigma^2_{bh} + 2\sigma^2_{bw} + 2r_{wh\cdot ww}\sigma_{wh}\sigma_{ww} + 2r'_{ww\cdot bh}\sigma_{ww}\sigma_{bh} + 2\sqrt{2}r''_{bh\cdot bw}\sigma_{bh}\sigma_{bw} \quad [10]$$

To get the systematic organization of terms needed in such a presentation we have followed the rule of putting heredity terms before environmental ones, variance before covariance, within-family before between-family. The primes and double primes on r's are explained above. Occasionally, where a mathematically *possible* combinatory term might also seem *needed* by the reader, but has been dropped by us on fuller consideration, it has been placed in brackets, i.e., strictly we regard the term as unnecessary. We have deliberately refrained from working out as decimals the various numerical coefficients, since their representation as radicals enables the reader more easily to perceive their derivation.

The equations of Table 1 are presented as a totality of experimentally available equations, from which an experimenter may draw according to the research resources available to him and the degree of simplification of assumptions which he cares to make. Even the Complete Design cannot simultaneously use all of them, because of the linear dependencies noted above, though this does not avoid collecting the data that would be required to use virtually all of them.[6] The 11 equations which we advocate as most effective for the final determinations of all 11 unknowns in 16 are: 1, 2, 3, 5, 6, 7, 8, 9, 10, 11, and 14. The only regrettable waste in this choice is that it does not use sibs raised together, Equation 4, an easily obtainable group, but the alternatives are worse. For example, half-sibs raised together are needed in most others, and these are socially difficult to identify, with scientific reliability.

The costly features of the Complete Design are identical twins reared apart and half-sibs reared apart, both of which require much effort to locate. Accordingly we have suggested for most researchers the Limited Resources Design in Table 2, which uses only 10 equations and requires only sibs, and, in twins, those reared together. A sufficient sample of these should be locatable by any investigator near a large city. It is not

[6] Commonly, one has the choice of using either the within or the between variance from data gathered on a particular type of familial group.

possible to create this design simply by lifting 10 equations from Table 1; for the assumptions that are made in each have to be changed in order to be able to reduce the total number of equations. The simpler assumptions, (*a*) consider the environmental variance for identical and fraternal twins raised together to be identical, as $\sigma^2_{we'}$; (*b*) omit differentiation of $r'_{wh.we}$ and $r_{wh.we}$; i.e., the modification of the environment–heredity correlation when the hereditary and the environmental variance are increased in families, respectively, of unrelated children raised together and sibs raised apart. Thus it solves for five variances and three correlations, instead of for six variances and five correlations as in the Complete Design.

THE SOLUTIONS AND THEIR CONFIDENCE LIMITS

Since the solutions of the above equations are a considerable undertaking—mainly because of the need to choose the best sequences in combining them—it would be helpful to have tables set out for the general solutions. The results of some three or four months of algebraic work and checking are set out in Tables 3 and 4, in which the researcher can simply insert his experimentally-obtained, concrete variances to obtain the abstract variances. In order not to interrupt the main presentation these have been placed at the end of the article. Quite apart from the substantial algebraic task there are certain theoretical problems in solving the equations of Table 1, which have been discussed in another context by Nelder (1953). Some of these are approached below.

Additionally to the solutions in Tables 3 and 4, which are only as reliable as human pursuit of long manipulations of simultaneous equations can be, and should accordingly be checked before use, solutions have been worked out along different lines on the electronic computer. A matrix, A, consisting of the coefficients for variances and covariances arranged with rows corresponding to the 11 equations from the Complete Design, is post-multiplied by a Vector X of abstract variances and covariances to yield the vector of concrete variances, B, thus

$$AX = B$$

Solving for X would give the abstract variances, thus,

$$X = A^{-1}B$$

But, since we wish to compare the coefficients for the concrete variances in these solutions with those in Tables 3 and 4, which have to be utilized in the following discussion on standard errors and confidence limits, we stopped the solution short at obtaining A^{-1}. This was done by the following transformation because the Limited Resources Design, unlike the Complete Design, does not present a square matrix of coefficients

$$A^{-1} = (A'A)^{-1}A'$$

These matrices of coefficients may be obtained by application to the author.

The principal incompleteness of the MAVA method as first applied (Cattell, Blewett, & Beloff, 1955) to psychological data was the lack of standard errors and confidence limits for the values obtained. As Tables 3 and 4 remind one, each of the abstract variances finally obtained is a function of quite a number of experimentally-determined variances, while the correlation coefficients have at least an equally complex derivation.

At the present juncture in research it becomes important to attempt to solve the complex problem here presented. An additional complexity

TABLE 3

SOLUTIONS FOR SIX ABSTRACT VARIANCES AND FIVE HEREDITY-ENVIRONMENT CORRELATIONS, IN THE COMPLETE DESIGN

Note.—For convenience, certain equations below, namely, those having a quadratic solution, are set out in triplicate. The first statement shows the roots which give possible positive and negative entries, while the following "a" and "b" equations give the actual resulting alternative answers—and in decimals since there is no longer need to reveal the mode of derivation. In these cases both solutions are retained until further considerations cause one to be ruled out.

$$\sigma^2_{ww} = 2\sigma^2_{UA} - .5\sqrt{2}\sigma^2_{BITTF} - \sigma^2_{SA} + .5\sqrt{2}\sigma^2_{BITAF} - .5\sqrt{2}\sigma^2_{BBF} + \sigma^2_{ITA} + (.5\sqrt{2}-1)\sigma^2_{BNF} \quad [1]$$

$$\sigma^2_{ww} = 2\sigma^2_{UA} - .7070\sigma^2_{BITTF} - \sigma^2_{SA} + .7070\sigma^2_{BITAF} - .7070\sigma^2_{BBF} + \sigma^2_{ITA} - .2930\sigma^2_{BNF} \quad [1a]$$

$$\sigma^2_{ww} = 2\sigma^2_{UA} + .7070\sigma^2_{BITTF} - \sigma^2_{SA} - .7070\sigma^2_{BITAF} + .7070\sigma^2_{BBF} + \sigma^2_{ITA} - 1.7070\sigma^2_{BNF} \quad [1b]$$

$$\sigma^2_{ww'} = 2r^2_{wh\cdot ww}\sigma^2_{wh} - (\sigma^2_{wh} - \sigma^2_{FTT}) - r_{wh\cdot ww}\sqrt{4r^2_{wh\cdot ww}\sigma^4_{wh} - 4\sigma^2_{wh}\sigma^2_{FTT} + 4\sigma^2_{wh}\sigma^2_{FTT}} \quad [2]^a$$

$$\sigma^2_{ww''} = \sigma^2_{ITT} \quad [3]$$

$$\sigma^2_{wh} = (.5+.5\sqrt{2})\sigma^2_{BITTF} - .5\sqrt{2}\sigma^2_{SA} + .5\sigma^2_{BITAF} - .5\sigma^2_{BBF} - (.5+.5\sqrt{2})\sigma^2_{BNF} + .5\sqrt{2}\sigma^2_{ITA} \quad [4]$$

$$\sigma^2_{wh} = 1.2070\sigma^2_{BITTF} - .7070\sigma^2_{SA} + .5\sigma^2_{BITAF} - .5\sigma^2_{BBF} - 1.2070\sigma^2_{BNF} + .7070\sigma^2_{ITA} \quad [4a]$$

$$\sigma^2_{wh} = -.2070\sigma^2_{BITTF} + .7070\sigma^2_{SA} + .5\sigma^2_{BITAF} - .5\sigma^2_{BBF} + .2070\sigma^2_{BNF} - .7070\sigma^2_{ITA} \quad [4b]$$

$$\sigma^2_{bb} = .5\sqrt{2}\sigma^2_{BITTF} + \sigma^2_{SA} - 2\sigma^2_{UA} - .5\sqrt{2}\sigma^2_{BITAF} + .5\sqrt{2}\sigma^2_{BBF} + (1-.5\sqrt{2})\sigma^2_{BNF} \quad [5]$$

$$\sigma^2_{bb} = .7070\sigma^2_{BITTF} + \sigma^2_{SA} - 2\sigma^2_{UA} - .7070\sigma^2_{BITAF} + .7070\sigma^2_{BBF} + .2930\sigma^2_{BNF} \quad [5a]$$

$$\sigma^2_{bb} = -.7070\sigma^2_{BITTF} + \sigma^2_{SA} - 2\sigma^2_{UA} + .7070\sigma^2_{BITAF} - .7070\sigma^2_{BBF} + 1.7070\sigma^2_{BNF} \quad [5b]$$

$$\sigma^2_{bh} = \sigma^2_{BITTF} + \sqrt{2}\sigma^2_{SA} + (2+\sqrt{2})\sigma^2_{UA} - \sigma^2_{BITAF} + \sigma^2_{BBF} - \sigma^2_{BNF} - (2\sqrt{2}+2)\sigma^2_{HSA} \quad [6]$$

$$\sigma^2_{bh} = \sigma^2_{BITTF} + 1.4141\sigma^2_{SA} + 3.4141\sigma^2_{UA} - \sigma^2_{BITAF} + \sigma^2_{BBF} - \sigma^2_{BNF} - 4.8282\sigma^2_{HSA} \quad [6a]$$

$$\sigma^2_{bh} = \sigma^2_{BITTF} - 1.4141\sigma^2_{SA} + .5859\sigma^2_{UA} - \sigma^2_{BITAF} + \sigma^2_{BBF} - \sigma^2_{BNF} + .8282\sigma^2_{HSA} \quad [6b]$$

$$r_{wh\cdot ww} = \frac{.25\sigma^2_{BNF} - (.25\sqrt{2}+.5)\sigma^2_{ITA} - .25\sigma^2_{BITTF} + (.25\sqrt{2}+.5)\sigma^2_{SA}}{\sqrt{(.5+.5\sqrt{2})\sigma^2_{BITTF} - .5\sqrt{2}\sigma^2_{SA} + .5\sigma^2_{BITAF} - .5\sigma^2_{BBF} - (.5+.5\sqrt{2})\sigma^2_{BNF} + .5\sqrt{2}\sigma^2_{ITA}}}$$
$$\times \frac{-(.25\sqrt{2}+.25)\sigma^2_{BITAF} + (.25\sqrt{2}+.25)\sigma^2_{BBF}}{\sqrt{2\sigma^2_{UA} - .5\sqrt{2}\sigma^2_{BITTF} - \sigma^2_{SA} + .5\sqrt{2}\sigma^2_{BITAF} - .5\sqrt{2}\sigma^2_{BBF} + \sigma^2_{ITA} + (.5\sqrt{2}-1)\sigma^2_{BNF}}} \quad [7]$$

$$r'_{wh\cdot ww} = \frac{(.25\sqrt{2}+.5)\sigma^2_{SA} - (.25\sqrt{2}+.5)\sigma^2_{BSF} + (.25\sqrt{2}+.25)\sigma^2_{BNF} - (.25\sqrt{2}+.25)\sigma^2_{BITTF}}{\sqrt{(.5+.5\sqrt{2})\sigma^2_{BITTF} - .5\sqrt{2}\sigma^2_{SA} + .5\sigma^2_{BITAF} - .5\sigma^2_{BBF} - (.5+.5\sqrt{2})\sigma^2_{BNF} + .5\sqrt{2}\sigma^2_{ITA}}}$$
$$\times \frac{-.25\sigma^2_{BITAF} + .25\sigma^2_{BBF}}{\sqrt{2\sigma^2_{UA} - .5\sqrt{2}\sigma^2_{BITTF} - \sigma^2_{SA} + .5\sqrt{2}\sigma^2_{BITAF} - .5\sqrt{2}\sigma^2_{BBF} + \sigma^2_{ITA} + (.5\sqrt{2}-1)\sigma^2_{BNF}}} \quad [8]$$

$$r'_{ww\cdot bh} = \frac{(\sqrt{2}+1)\sigma^2_{HSA} + (1-.25\sqrt{2})\sigma^2_{BNF} - .5\sqrt{2}\sigma^2_{SA} - (.2+.5\sqrt{2})\sigma^2_{UA}}{\sqrt{2\sigma^2_{UA} - .5\sqrt{2}\sigma^2_{BITTF} - \sigma^2_{SA} + .5\sqrt{2}\sigma^2_{BITAF} - .5\sqrt{2}\sigma^2_{BBF} + \sigma^2_{ITA} + (.5\sqrt{2}-1)\sigma^2_{BNF}}}$$
$$\times \frac{+(.5-.25\sqrt{2})\sigma^2_{BITAF} + (.25\sqrt{2}-.5)\sigma^2_{BITTF} + (.25\sqrt{2}-.5)\sigma^2_{BBF} + .5\sigma^2_{UT}}{\sqrt{\sigma^2_{BITTF} + 2\sigma^2_{SA} + (2+\sqrt{2})\sigma^2_{UA} - \sigma^2_{BITAF} + \sigma^2_{BBF} - \sigma^2_{BNF} - (2\sqrt{2}+2)\sigma^2_{HSA}}} \quad [9]$$

[a] For the sake of simplification it is best to determine values for $r_{wh\cdot ww}$ and σ^2_{wh} and substitute these in this equation to determine $\sigma^2_{ww'}$.

[b] Solutions 12 through 16 are not directly for any required end value—there are only 11 real unknowns. They are introduced as conveniences in the process of solution. They consist of phrases containing radicals, and were treated as single unknowns in the solution of the original equations. The correlations $r_{wh\cdot ww}$ etc., can be directly obtained for these solutions by dividing the solution obtained by the product of the corresponding squares of the variances indicated on the left-hand side of the equation. Or, if it is desired the four solutions (7–11) may be used and by direct substitution, the values for $r_{wh\cdot ww}$, etc., can be obtained. In Equations 7 through 11, only one solution is given to save time and space. (If the plus value of 2 is used in obtaining the variances (Equations 1–6) consistency must be followed by using plus values throughout.

TABLE 3—*Continued*

$$r_{bh\cdot be}=\frac{(.5+.25\sqrt{2})\sigma^2_{BITAF}-(.5+.25\sqrt{2})\sigma^2_{BITTF}-(.5+.5\sqrt{2})\sigma^2_{UA}-(.5+.25\sqrt{2})\sigma^2_{BBF}}{\sqrt{.5\sqrt{2}\sigma^2_{BITTF}+\sigma^2_{SA}-2\sigma^2_{UA}-.5\sqrt{2}\sigma^2_{BITAF}+.5\sqrt{2}\sigma^2_{BBF}+(1-.5\sqrt{2})\sigma^2_{BNF}}}$$

$$\times\frac{-(.5+.5\sqrt{2})\sigma^2_{SA}+(\sqrt{2}+1)\sigma^2_{HSA}+(.5+.25\sqrt{2})\sigma^2_{BNF}}{\sqrt{\sigma^2_{BITTF}+\sqrt{2}\sigma^2_{SA}+(2+\sqrt{2})\sigma^2_{UA}-\sigma^2_{BITAF}+\sigma^2_{BBF}-\sigma^2_{BNF}-(2\sqrt{2}+2)\sigma^2_{HSA}}} \quad [10]$$

$$r''_{bh\cdot be}=\frac{\sigma^2_{HSA}+(.75\sqrt{2}-.75)\sigma^2_{BNF}+(.25\sqrt{2}-1)\sigma^2_{SA}+(1-.5\sqrt{2})\sigma^2_{UA}+(.75\sqrt{2}-.25)\sigma^2_{BITAF}}{\sqrt{.5\sqrt{2}\sigma^2_{BITTF}+\sigma^2_{SA}-2\sigma^2_{UA}-.5\sqrt{2}\sigma^2_{BITAF}+.5\sqrt{2}\sigma^2_{BBF}+(1-.5\sqrt{2})\sigma^2_{BNF}}}$$

$$\times\frac{+(.25-.75\sqrt{2})\sigma^2_{BITTF}+(.25-.5\sqrt{2})\sigma^2_{BBF}-.5\sigma^2_{UT}}{\sqrt{\sigma^2_{BITTF}+\sqrt{2}\sigma^2_{SA}+(2+\sqrt{2})\sigma^2_{UA}-\sigma^2_{BITAF}+\sigma^2_{BBF}-\sigma^2_{BNF}-(2\sqrt{2}+2)\sigma^2_{HSA}}} \quad [11]$$

$$r_{wh\cdot we}\sigma_{wh}\sigma_{we}=.25\sigma^2_{BNF}-(.25\sqrt{2}+.5)\sigma^2_{ITA}-.25\sigma^2_{BITTF}+(.25\sqrt{2}+.5)\sigma^2_{SA}$$
$$-(.25\sqrt{2}+.25)\sigma^2_{BITAF}+(.25\sqrt{2}+.25)\sigma^2_{BBF} \quad [12]^b$$

$$r_{wh\cdot we}\sigma_{wh}\sigma_{we}=.25\sigma^2_{BNF}-.8535\sigma^2_{ITA}-.25\sigma^2_{BITTF}+.8535\sigma^2_{SA}-.8535\sigma^2_{BITAF}+.8535\sigma^2_{BBF} \quad [12a]$$

$$r_{wh\cdot we}\sigma_{wh}\sigma_{we}=.25\sigma^2_{BNF}-.1465\sigma^2_{ITA}-.25\sigma^2_{BITTF}+.1465\sigma^2_{SA}-.1465\sigma^2_{BITAF}+.1465\sigma^2_{BBF} \quad [12b]$$

$$r'_{wh\cdot we}\sigma_{wh}\sigma_{we}=(.25\sqrt{2}+.5)\sigma^2_{SA}-(.25\sqrt{2}+.5)\sigma^2_{BSF}+(.25\sqrt{2}+.25)\sigma^2_{BNF}$$
$$-(.25\sqrt{2}+.25)\sigma^2_{BITTF}-.25\sigma^2_{BITAF}+.25\sigma^2_{BBF} \quad [13]$$

$$r'_{wh\cdot we}\sigma_{wh}\sigma_{we}=.8535\sigma^2_{SA}-.8535\sigma^2_{BSF}+.6035\sigma^2_{BNF}-.6035\sigma^2_{BITTF}-.25\sigma^2_{BITAF}+.25\sigma^2_{BBF} \quad [13a]$$

$$r'_{wh\cdot we}\sigma_{wh}\sigma_{we}=.1465\sigma^2_{SA}-.1465\sigma^2_{BSF}-.1035\sigma^2_{BNF}+.1035\sigma^2_{BITTF}-.25\sigma^2_{BITAF}+.25\sigma^2_{BBF} \quad [13b]$$

$$r'_{we\cdot bh}\sigma_{we}\sigma_{bh}=(\sqrt{2}+1)\sigma^2_{HSA}+(1-.25\sqrt{2})\sigma^2_{BNF}-.5\sqrt{2}\sigma^2_{SA}-(2+.5\sqrt{2})\sigma^2_{UA}$$
$$+(.5-.25\sqrt{2})\sigma^2_{BITAF}+(.25\sqrt{2}-.5)\sigma^2_{BITTF}+(.25\sqrt{2}-.5)\sigma^2_{BBF}+.5\sigma^2_{UT} \quad [14]$$

$$r'_{we\cdot bh}\sigma_{we}\sigma_{bh}=2.4141\sigma^2_{HSA}+.6565\sigma^2_{BNF}-.7070\sigma^2_{SA}-2.7070\sigma^2_{UA}+.1465\sigma^2_{BITAF}$$
$$-.1465\sigma^2_{BITTF}-.1465\sigma^2_{BBF}+.5\sigma^2_{UT} \quad [14a]$$

$$r'_{we\cdot bh}\sigma_{we}\sigma_{bh}=-.4141\sigma^2_{HSA}+1.3535\sigma^2_{BNF}+.7070\sigma^2_{SA}-1.2930\sigma^2_{UA}+.8535\sigma^2_{BITAF}$$
$$-.8535\sigma^2_{BITTF}-.8535\sigma^2_{BBF}+.5\sigma^2_{UT} \quad [14b]$$

$$r_{bh\cdot be}\sigma_{bh}\sigma_{be}=(.5+.25\sqrt{2})\sigma^2_{BITAF}-(.5+.25\sqrt{2})\sigma^2_{BITTF}-(.5+.5\sqrt{2})\sigma^2_{UA}$$
$$-(.5+.25\sqrt{2})\sigma^2_{BBF}-(.5+.5\sqrt{2})\sigma^2_{SA}+(\sqrt{2}+1)\sigma^2_{HSA}+(.5+.25\sqrt{2})\sigma^2_{BNF} \quad [15]$$

$$r_{bh\cdot be}\sigma_{bh}\sigma_{be}=.8535\sigma^2_{BITAF}-.8535\sigma^2_{BITTF}-1.2070\sigma^2_{UA}-.8535\sigma^2_{BBF}-1.2070\sigma^2_{SA}$$
$$+2.4141\sigma^2_{HSA}+.8535\sigma^2_{BNF} \quad [15a]$$

$$r_{bh\cdot be}\sigma_{bh}\sigma_{be}=+.1465\sigma^2_{BITAF}-.1465\sigma^2_{BITTF}+.2070\sigma^2_{UA}-.1465\sigma^2_{BBF}+.2070\sigma^2_{SA}$$
$$-.4141\sigma^2_{HSA}+.4650\sigma^2_{BNF} \quad [15b]$$

$$r''_{bh\cdot be}\sigma_{bh}\sigma_{be}=\sigma^2_{HSA}+(.75\sqrt{2}-.75)\sigma^2_{BNF}+(.25\sqrt{2}-1)\sigma^2_{SA}+(1-.5\sqrt{2})\sigma^2_{UA}$$
$$+(.75\sqrt{2}-.25)\sigma^2_{BITAF}+(.25-.75\sqrt{2})\sigma^2_{BITTF}+(.25-.5\sqrt{2})\sigma^2_{BBF}-.5\sigma^2_{UT} \quad [16]$$

$$r''_{bh\cdot be}\sigma_{bh}\sigma_{be}=\sigma^2_{HSA}+.3106\sigma^2_{BNF}-.6465\sigma^2_{SA}+.2930\sigma^2_{UA}+.8106\sigma^2_{BITAF}$$
$$-.8106\sigma^2_{BITTF}-.4570\sigma^2_{BBF}-.5\sigma^2_{UT} \quad [16a]$$

$$r''_{bh\cdot be}\sigma_{bh}\sigma_{be}=\sigma^2_{HSA}-1.8106\sigma^2_{BNF}-1.3535\sigma^2_{SA}+1.7070\sigma^2_{UA}-1.3106\sigma^2_{BITAF}$$
$$+1.3106\sigma^2_{BITTF}+.9570\sigma^2_{BBF}-.5\sigma^2_{UT} \quad [16b]$$

lies in the fact that most investigators (including the present writer), have usually not been content with the values for the four main variances but have proceeded to calculate various ratios between these, or between certain sums of these, e.g., the ratio of *total* environmental to *total* hereditary variance. For these final *ratios* are more likely to express the ultimate

TABLE 4

SOLUTIONS FOR THE FIVE VARIANCES AND THREE CORRELATIONS IN THE LIMITED RESOURCES DESIGN

$$\sigma^2_{ue} = (2-\sqrt{2})\sigma^2_{ST}+\sigma^2_{SA}-\sigma^2_{UT}+(2\sqrt{2}-2)\sigma^2_{UA}-\sigma^2_{BITTF}+(2-\sqrt{2})\sigma^2_{BNF}+\sigma^2_{BBF}-\sigma^2_{BSF} \quad [1]^a$$

$$\sigma^2_{ue} = .5859\sigma^2_{ST}+\sigma^2_{SA}-\sigma^2_{UT}+.8282\sigma^2_{UA}-\sigma^2_{BITTF}+.5859\sigma^2_{BNF}+\sigma^2_{BBF}-\sigma^2_{BSF} \quad [1a]$$

$$\sigma^2_{ue} = 3.4141\sigma^2_{ST}+\sigma^2_{SA}-\sigma^2_{UT}-4.8181\sigma^2_{UA}-\sigma^2_{BITTF}+3.4141\sigma^2_{BNF}+\sigma^2_{BBF}-\sigma^2_{BSF} \quad [1b]$$

$$\sigma^2_{ue'} = \sigma^2_{ITT} \quad [2]$$

$$\sigma^2_{uh} = \sigma^2_{ST}(\sqrt{2}-1)+.5\sqrt{2}\sigma^2_{SA}-.5\sqrt{2}\sigma^2_{UT}+(2-2\sqrt{2})\sigma^2_{UA}+\sigma^2_{BITTF} + (\sqrt{2}-2)\sigma^2_{BNF}+.5\sqrt{2}\sigma^2_{BBF}-.5\sqrt{2}\sigma^2_{BSF} \quad [3]$$

$$\sigma^2_{uh} = .4141\sigma^2_{ST}+.7070\sigma^2_{SA}-.7070\sigma^2_{UT}-.8282\sigma^2_{UA}+\sigma^2_{BITTF}-.5859\sigma^2_{BNF} + .7070\sigma^2_{BBF}-.7070\sigma^2_{BSF} \quad [3a]$$

$$\sigma^2_{uh} = -2.4141\sigma^2_{ST}-.7070\sigma^2_{SA}+.7070\sigma^2_{UT}+4.8282\sigma^2_{UA}+\sigma^2_{BITTF} - 3.4141\sigma^2_{BNF}-.7070\sigma^2_{BBF}+.7070\sigma^2_{BSF} \quad [3b]$$

$$\sigma^2_{he} = \sigma^2_{SA}-2\sigma^2_{UA}+\sigma^2_{BNF} \quad [4]$$

$$\sigma^2_{hh} = (1+.5\sqrt{2})\sigma^2_{SA}+(1+.5\sqrt{2})\sigma^2_{BBF}-2\sigma^2_{UA}+\sigma^2_{BNF}-(1+.5\sqrt{2})\sigma^2_{BSF}-.5\sqrt{2}\sigma^2_{UT} \quad [5]$$

$$\sigma^2_{hh} = 1.7070\sigma^2_{SA}+1.7070\sigma^2_{BBF}-2\sigma^2_{UA}+\sigma^2_{BNF}-1.7070\sigma^2_{BSF}-.7070\sigma^2_{UT} \quad [5a]$$

$$\sigma^2_{hh} = .2930\sigma^2_{SA}+.2930\sigma^2_{BBF}-2\sigma^2_{UA}+\sigma^2_{BNF}-.2930\sigma^2_{BSF}+.7070\sigma^2_{UT} \quad [5b]$$

$$r_{uh\cdot ue} = \frac{.5\sigma^2_{FTT}-.5\sigma^2_{ITT}-.5\sigma^2_{BITTF}-.25\sqrt{2}\sigma^2_{SA}-.25\sqrt{2}\sigma^2_{BBF}}{\sqrt{\sigma^2_{SA}(\sqrt{2}-1)+.5\sqrt{2}\sigma^2_{SA}-.5\sqrt{2}\sigma^2_{UT}+(2-2\sqrt{2})\sigma^2_{UA}+\sigma^2_{BITTF}}}$$

(Square root sign continued)

$$\frac{+.25\sqrt{2}\sigma^2_{BSF}+(1-.5\sqrt{2})\sigma^2_{BNF}+(\sqrt{2}-1)\sigma^2_{UA} + (\sqrt{2}-2)\sigma^2_{BNF}+.5\sqrt{2}\sigma^2_{BBF}-.5\sqrt{2}\sigma^2_{BSF}}{\times\sqrt{(2-\sqrt{2})\sigma^2_{ST}+\sigma^2_{SA}-\sigma^2_{UT}+(2\sqrt{2}-2)\sigma^2_{UA}-\sigma^2_{BITTF}+(2-\sqrt{2})\sigma^2_{BNF}+\sigma^2_{BBF}-\sigma^2_{BSF}}} \quad [6]$$

$$r'_{ue\cdot hh} = \frac{+(.5-.5\sqrt{2})\sigma^2_{ST}+(.25\sqrt{2})\sigma^2_{UT}}{\sqrt{(2-\sqrt{2})\sigma^2_{ST}+\sigma^2_{SA}-\sigma^2_{UT}+(2\sqrt{2}-2)\sigma^2_{UA}-\sigma^2_{BITTF}+(2-\sqrt{2})\sigma^2_{BNF}+\sigma^2_{BBF}-\sigma^2_{BSF}}}$$

$$\times\frac{.25\sigma^2_{UT}-.25\sigma^2_{SA} - .25\sigma^2_{BBF}+.25\sigma^2_{BSF}}{\sqrt{(1+.5\sqrt{2})\sigma^2_{SA}+(1+.5\sqrt{2})\sigma^2_{BBF}-2\sigma^2_{UA}+\sigma^2_{BNF}-(1+.5\sqrt{2})\sigma^2_{BSF}-.5\sqrt{2}\sigma^2_{UT}}} \quad [7]$$

$$r_{hh\cdot he} = \frac{.25\sqrt{2}\sigma^2_{UT}-(1+.25\sqrt{2})\sigma^2_{SA}+1.5\sigma^2_{UA}-.5\sigma^2_{BNF}}{\sqrt{\sigma^2_{SA}-2\sigma^2_{UA}+\sigma^2_{BNF}}}$$

$$\times\frac{-(.25\sqrt{2}+.5)\sigma^2_{BBF}+(.25\sqrt{2}+.5)\sigma^2_{BSF}}{\sqrt{(1+.5\sqrt{2})\sigma^2_{SA}+(1+.5\sqrt{2})\sigma^2_{BBF}-2\sigma^2_{UA}+\sigma^2_{BNF}-(1+.5\sqrt{2})\sigma^2_{BSF}-.5\sqrt{2}\sigma^2_{UT}}} \quad [8]$$

$$r''_{hh\cdot he} = \frac{\sqrt{2}\sigma^2_{UA}-.5\sqrt{2}\sigma^2_{SA}-.5\sqrt{2}\sigma^2_{BNF}}{\sqrt{\sigma^2_{SA}-2\sigma^2_{UA}+\sigma^2_{BNF}}}$$

[a] These variances (Subequations a and b) are given solutions wherein $\sqrt{2}$ assumes a value of 1.4141 in 1a throughout, and wherein $\sqrt{2}$ assumes a value of -1.4141 in 1b.

[b] Solutions 10 through 14 are for phrases containing radicals. These phrases were treated, as in Table 1, as single unknowns in the solution of the original equations. The correlations $r_{uh\cdot ue}$ etc., can be directly obtained from these solutions by dividing the solution obtained by the product of the corresponding squares of the variances indicated on the left-hand side of the equation. Or, if it is desired the four solutions (6–9) may be used and by direct substitution, the values for $r_{uh\cdot ue}$ etc., can be obtained. In Equations 6 through 9, only one solution is given to save time and space. (If the plus values of 2 are used in obtaining the variances (Equations 1–5) consistency must be followed by using plus values throughout.)

TABLE 4—*Continued*

$$+.25\sqrt{2}\sigma^2_{BSF}-.25\sqrt{2}\sigma^2_{UT}$$
$$\times\sqrt{(1+.5\sqrt{2})\sigma^2_{SA}+(1+.5\sqrt{2})\sigma^2_{BBF}-2\sigma^2_{UA}+\sigma^2_{BNF}-(1+.5\sqrt{2})\sigma^2_{BSF}-.5\sqrt{2}\sigma^2_{UT}} \quad [9]$$

$$r_{wh\cdot wc}\sigma_{wh}\sigma_{wc'} = .5\sigma^2_{FTT}-.5\sigma^2_{ITT}-.5\sigma^2_{BITTF}-.25\sqrt{2}\sigma^2_{SA}-.25\sqrt{2}\sigma^2_{BBF}+.25\sqrt{2}\sigma^2_{BSF}$$
$$+(1-.5\sqrt{2})\sigma^2_{BNF}+(\sqrt{2}-1)\sigma^2_{UA}+(.5-.5\sqrt{2})\sigma^2_{ST}+.25\sqrt{2}\sigma^2_{UT} \quad [10]^b$$

$$r_{wh\cdot wc}\sigma_{wh}\sigma_{wc'} = .5\sigma^2_{FTT}-.5\sigma^2_{ITT}-.5\sigma^2_{BITTF}-.3535\sigma^2_{SA}-.3535\sigma^2_{BBF}+.3535\sigma^2_{BSF}$$
$$+.2930\sigma^2_{BNF}+.4141\sigma^2_{UA}-.2070\sigma^2_{ST}+.3535\sigma^2_{UT} \quad [10a]$$

$$r_{wh\cdot wc}\sigma_{wh}\sigma_{wc'} = .5\sigma^2_{FTT}-.5\sigma^2_{ITT}-.5\sigma^2_{BITTF}+.3535\sigma^2_{SA}+.3535\sigma^2_{BBF}-.3535\sigma^2_{BSF}$$
$$+1.7070\sigma^2_{BNF}-2.4141\sigma^2_{UA}+1.2070\sigma^2_{ST}-.3535\sigma^2_{UT} \quad [10b]$$

$$r_{wh\cdot wc}\sigma_{wh}\sigma_{wc} = -.5\sigma^2_{ST}+(.5+.25\sqrt{2})\sigma^2_{UT}-(.5+.25\sqrt{2})\sigma^2_{SA}-(.5+.25\sqrt{2})\sigma^2_{BBF}$$
$$+(.5+.25\sqrt{2})\sigma^2_{BSF}-.5\sigma^2_{BNF}+\sigma^2_{UA} \quad [11]$$

$$r_{wh\cdot wc}\sigma_{wh}\sigma_{wc} = -.5\sigma^2_{ST}+.8535\sigma^2_{UT}-.8535\sigma^2_{SA}-.8535\sigma^2_{BBF}+.8535\sigma^2_{BSF}-.5\sigma^2_{BNF}+\sigma^2_{UA} \quad [11a]$$

$$r_{wh\cdot wc}\sigma_{wh}\sigma_{wc} = -.5\sigma^2_{ST}+.1465\sigma^2_{UT}-.1465\sigma^2_{SA}-.1465\sigma^2_{BBF}+.1465\sigma^2_{BSF}-.5\sigma^2_{BNF}+\sigma^2_{UA} \quad [11b]$$

$$r'_{wc\cdot bh}\sigma_{wc}\sigma_{bh} = .25\sigma^2_{UT}-.25\sigma^2_{SA}-.25\sigma^2_{BBF}+.25\sigma^2_{BSF} \quad [12]$$

$$r_{bh\cdot bc}\sigma_{bh}\sigma_{bc} = .25\sqrt{2}\sigma^2_{UT}-(1+.25\sqrt{2})\sigma^2_{SA}+1.5\sigma^2_{UA}-.5\sigma^2_{BNF}$$
$$-(.25\sqrt{2}+.5)\sigma^2_{BBF}+(.25\sqrt{2}+.5)\sigma^2_{BSF} \quad [13]$$

$$r_{bh\cdot bc}\sigma_{bh}\sigma_{bc} = .3535\sigma^2_{UT}-1.3535\sigma^2_{SA}+1.5\sigma^2_{UA}-.5\sigma^2_{BNF}-.8535\sigma^2_{BBF}+.8535\sigma^2_{BSF} \quad [13a]$$

$$r_{bh\cdot bc}\sigma_{bh}\sigma_{bc} = -.3535\sigma^2_{UT}-.6465\sigma^2_{SA}+1.5\sigma^2_{UA}-.5\sigma^2_{BNF}-.1465\sigma^2_{BBF}+.1465\sigma^2_{BSF} \quad [13b]$$

$$r''_{bh\cdot bc}\sigma_{bh}\sigma_{bc} = \sqrt{2}\sigma^2_{UA}-.5\sqrt{2}\sigma^2_{SA}-.5\sqrt{2}\sigma^2_{BNF}+.25\sqrt{2}\sigma^2_{BBF}-.25\sqrt{2}\sigma^2_{UT} \quad [14]$$

$$r''_{bh\cdot bc}\sigma_{bh}\sigma_{bc} = 1.4141\sigma^2_{UA}-.7070\sigma^2_{SA}-.7070\sigma^2_{BNF}+.3535\sigma^2_{BSF}-.3535\sigma^2_{UT} \quad [14a]$$

$$r''_{bh\cdot bc}\sigma_{bh}\sigma_{bc} = -1.4141\sigma^2_{UA}+.7070\sigma^2_{SA}+.7070\sigma^2_{BNF}-.3535\sigma^2_{BSF}+.3535\sigma^2_{UT} \quad [14b]$$

inferences in which one is interested, as a starting point for further theoretical psychological developments and experiments. Yet determining the distribution and the confidence limits of ratios presents an additional challenge to statistical inference.

The typical form of any solution for an abstract variance, as in Tables 3 and 4 above, may be written:

$$\hat{\sigma}^2_a = k_1\sigma^2_1 + k_2\sigma^2_2 \cdots + k_n\sigma^2_n \quad [2]$$

where $\hat{\sigma}^2_a$ is the estimated abstract variance, e.g., within family environment; k_1, k_2, etc., are numerical, constants; and σ^2_1, etc., are the estimated concrete variances for particular family relation population species. Our need is to determine the variance of the variance on the left, and to do so it is necessary to decide whether the variances on the right are correlated. Usually within and between mean squares are obviously uncorrelated for any one particular analysis. In this case, since they are quite different groups of subjects (*Ss*), it might be argued that the chances of systematic correlation are still less. However, the question is actually a very complex one, to be answered only by study of the structure of the data in each case. For example, in some instances of sibling pairs, they are *not* random subgroups from the larger group. The concensus of expert statistical opinion consulted nevertheless is that, in terms of possible correlation over a series of samples, we are justified in treating these as uncorrelated variances. Accordingly the variance of the abstract variance estimate is a

simple sum of the independent functions of the experimentally obtained variance estimates on the right of our equations, i.e.,

$$\delta^2{}_{\sigma_a{}^2} = \frac{2\Sigma k_i{}^2\sigma_i{}^4}{n_i - 1} \qquad [3]$$

where i is any subgroup.

As stated above, it is likely in most actual experiments, unless one is prepared to throw away hard won data, that the numbers in the various experimentally obtained family constellation subgroups (sibs, twins, etc.) will differ. If the successive groups in Equation 1 have n_1, n_2, etc. cases the above estimate of the variance of the variance is stated more completely as:

$$\delta^2{}_{\sigma_a{}^2} = \frac{2k_1{}^2\sigma_1{}^4}{n_1 - 1} + \frac{2k_2{}^2\sigma_2{}^4}{n_2 - 1} \cdots + \frac{2k_n{}^2\sigma_n{}^4}{n_n - 1} \qquad [4]$$

However, if n is small—less than 30 cases (unlikely in these experiments) —the degrees of freedom which give an unbiased estimate become $n_i + 1$, etc., according to Daniels (1939) and Welch (1956).

Thus the confidence limits for a given abstract variance can be designated as:

$$Pr\left[\sigma_a{}^2 - Z_j\frac{\sqrt{2\Sigma k_i\sigma_i{}^2}}{n_i - 1} < \delta_a{}^2 < \sigma_a{}^2 + Z_j\frac{\sqrt{2\Sigma k_i\sigma_i{}^2}}{n_i - 1}\right] = P_j \qquad [5]$$

where Z_j is the normal standard deviate for a given probability, P_j; $\sigma_a{}^2$ the "true" value for the population variance, and we assume that, through large n or the use of an unbiased estimate, the distribution of the variance is normal.

Finally, to determine the confidence limits of heredity-environment variance ratio's, e.g., $\frac{\sigma_{bh}{}^2}{\sigma_{be}{}^2}$, or, in general $\frac{\sigma_a{}^2}{\sigma_b{}^2}$, we simplify the probability statements for numerator and denominator as follows:

$$Pr[A < \delta_a{}^2 < B] = P_1 \qquad [6a]$$

$$Pr[C < \delta_b{}^2 < D] = P_2 \qquad [6b]$$

The combined probability for the ratio is not strictly the product P_1P_2 because P_1 and P_2 are not entirely independent; but when using the extreme confidence interval ranges (.95 to .99) in which we are alone interested, the error from assumption of independence is very small[7] and we may use:

$$Pr\left[\frac{A}{B} < \frac{\delta_a{}^2}{\delta_b{}^2} < \frac{B}{C}\right] = P_1P_2 \qquad [7]$$

As with any confidence limits test, it is not possible to decide, without actual empirical variance data carrying experimental error, what the desirable number of cases should be in a well planned experiment. However, with such a major experimental undertaking as is involved in a really well designed MAVA experiment, to determine nature–nurture ratios for some particular primary personality source trait, obtaining the earliest possible indication from experience as to what is adequate is vital. Fortunately, a lead can now be obtained by applying the above formulae to the sample sizes achieved in the first investigations of inheritance of primary personality factors by Cattell, Blewett, and Beloff (1955) and Cattell, Kristy, and Stice (1957). These studies measured a total of 1,024 eleven- and twelve-year old children

[7] For example with both P_1 and P_2 equal to .90 and assuming independence we get $P_1P_2 = .81$, whereas assuming lack of independence this becomes .80.

on all personality factors established up to that time, using, first, questionnaire measures (the High School Personality Questionnaire) and, secondly, objective tests (the Children's Objective-Analytic Battery). Taking for illustration first a factor from the questionaire, and one with the highest of hereditary determinations (next to intelligence), we choose *H* factor, parmia vs. threctia, a temperament factor believed associated with parasympathetic system predominance. The value $\frac{\sigma_{wa}^2}{\sigma_{wk}^2}$ came out at $\frac{.73}{1.39} = .52$, making heredity about twice as important, within the family as environment. According to our present formulae above the $P = .05$ limits are approximately .18 and 1.39.

Taking as different a case as possible for our second illustration we turn to a predominantly environmentally determined factor and one measured in objective tests. Let us examine the between family variance. For this we chose UI 23 (Cattell, 1957) which Eysenck has called the general neuroticism factor and which we call ergic regression. The value of $\frac{\sigma_{ba}^2}{\sigma_{bk}^2}$ came out at 6.7, and the $P = .05$ limits by the above formulae stand at 1.9 and 32.0. All values make environment more important than heredity, but the range in this case is very large.

It is well known that as one extends the number of linear combinations from which estimations are made the magnitude of the standard error increases in the disheartening fashion seen in the above results. However, the conclusion to be drawn, since knowledge of the relative environmental and hereditary contributions on continuous personality and ability dimensions is of great theoretical and practical importance (and since the twin method has similarly large sampling errors and is additionally, fundamentally amiss in its assumptions!) is surely that research must foot the bill. To say that the method is self-defeating because it requires 10,000 rather than 1,000 cases is to use an argument which, applied to the analogous wearisome organization required in separating radium, would have brought the work of the Curies to a standstill, or, in our own generation, would have blocked the way to atomic energy. Those who believe that advance in the human sciences is as important as in the physical sciences will not blink at the expenditure which, though large compared with a rat experiment, is still a hundredth of that in several current physical science problems. At present (unless one pursues the mirage of analogy from animal experimentation) the MAVA method, despite its experimental-organizational arduousness, is the only way either to check basic theories or, what is more important, generate more exact and potent hypotheses, as discussed below. Nevertheless, it is presented here as unfinished in development, and requiring improvements, notably in refinement of the estimation of standard errors, in the use of the attenuation correction, and in regard to the assumption of noncorrelation in Equation 7, etc. These issues would require separate papers in themselves.

While no space can be given here to heading off beforehand the numerous misunderstandings possible on so complex a topic, we would point out that perusal of the discussion of assumptions in their psychological context in the earlier articles (Cattell, 1953; Cattell, Blewett, & Beloff, 1955; Cattell, Kristy, & Stice, 1957) will make the present statements of for-

mulae, etc., more meaningful to the psychologist.

PSYCHOLOGICAL THEORY IMPLICIT IN THE MAVA MODEL

What has been propounded above is not a theory, in the sense of most discussed in this *Review*, but a model Burt and Howard (1957), and a few others on intelligence, and of Cattell, Beloff, and Blewett (1955), and Cattell, Kristy, and Stice (1957) on personality, nothing existed to restrict and a method, by which a considerable array of theories can be expressed and tested. In this area the vital need of psychology has been the initial appearance of some dependable relationships out of which a theory worthy of that name, rather than mere speculation, can be born. Apart from the work of Burks (1928), Burt and Howard (1957), and a few others on intelligence, and of Cattell, Beloff, and Blewett (1955), and Cattell, Kristy, and Stice (1957) on personality, nothing existed to restrict speculators from ascribing anything from 0.1% to 99.9% of the variance in any human trait to whichever influence their prejudices favored.

The restrictions which the present model puts upon theories (additive hereditary and environmental effects, interaction of heredity and environment by correlation and covariance but not statistical interaction) are slight compared to the aid it gives in checking many theories. Among the theories deserving experimental investigation are:

1. The position adopted in our own investigations that the variance ratio for any factorially composite piece of behavior will be middling but that more clear-cut predominance of heredity or environment will be found for factor-pure source traits (Cattell, 1946). Although it is theoretically possible, as mooted by Thomson and the present writer (1946) that a genetic factor (gene) may appear as a statistical factor it is almost certain that constitutional factors among the psychologists factors will be polygenic. Nevertheless there is a tendency for factors to be predominantly either environmental or hereditary (Cattell, Kristy, & Stice, 1957).

2. The theory that the occurrence of environmental influences at particular (early) ages, as in Freudian theory or in imprinting (Hess, 1959), is more effective than at others. In animals this theory can be tested by manipulative experiment, but in humans only by some form of the MAVA method applied to groups with different histories. In this connection an editorial critic has asked, "How can one possibly separate maturation from learning, age decline, etc., when these operate differently at different ages?" The answer is that it is precisely through, and only through, first obtaining these ratios for strategically chosen age levels, cultures, etc., that further inferences can be made regarding the periods of life, or the subcultural influences, determining the relative action of maturation and learning. For example, it was only through the discovery of the predominantly negative value of the heredity–environment *correlations*, from the above experiments, that the "law of coercion to the bio-social mean" (see 6 below) became justified as a worthwhile proposition regarding heredity–environment *interaction*.

3. The theory that, contrary to popular thinking, the ratio of hereditary influence will be higher with older *S*s. This assumes that younger children will have been exposed to a smaller sample of the total environment and will experience in this exposure the greater variance of means characteristic of smaller samples. The

hereditary variance remaining unchanged, the nature–nurture ratio should therefore be greater when older *S*s are used in the MAVA experiment.

4. A number of interesting theories are frequently entertained about the effects of family positions and constellations, of age differences between sibs and between children and parents, upon personality traits. Such theories can be systematically investigated by determining the relative environmental contributions for strategically chosen groups of *S*s in repeated MAVA experiments.

5. The importance of a general "family atmosphere," e.g., in the genesis of neurosis, can be checked through the comparison of within-family and between-family environmental variance on the MAVA applied to measures of neuroticism factors.

6. A variety of theories needs to be tested in regard to the causal directions in discovered correlations of heredity and environment. Does a positive correlation, for intelligence, mean that brighter children create a richer environment or that for social reasons they tend to be born into one? This can be answered partly by the comparison of $r_{be.bh}$ and $r_{we.wh}$. Does the large negative correlation of heredity and environment of the ergic regression factor, UI 23, (Eysenck's "neuroticism factor") require a theory that the neurosis-prone avoid the stresses that might precipitate neurosis? The general finding that hereditary and environmental influences tend to be negatively correlated with the more substantially inherited traits has already been the source of a theory (Cattell, 1957) of "cultural coercion to the bio-social norm."

7. A variety of theories in cultural anthropology and social psychology can now be examined through a comparison of the MAVA experimental results *in different countries*. For example, a democratic organization of society, providing opportunity for all, should raise the importance of heredity relative to environment in determining, for example, performance on intelligence tests.

8. The model also permits examination of theories regarding the effect of one trait upon another. A constitutional endowment of high schizothymia (*A* factor), for example, is to be considered part of the environment for another trait in the same individual, e.g., high intelligence. A comparison of MAVA findings on intelligence ratios for two types of *S*s—those preselected to be high and low, respectively, in *A* factor—can test and shape initial theories.

9. For the personality psychologist the most important theories of all are those concerning the hereditary or environmental origins of particular personality factors. For example, the finding that the *H* primary in the second-order extraversion factor (Cattell & Scheier, 1960) is largely hereditarily determined whereas *F*, surgency-desurgency, is equally a complete product of environmental learning situations, throws considerable new light on extraversion. Also, at one stroke, it cuts the theories to be investigated regarding *H* to largely physiological ones, and those regarding surgency to various possibilities in learning theory. A lot of theoretical wild goose chases are avoided. Equally theoretically provocative is the preliminary finding that Comention, UI 20, which some have called a superego pattern, is actually heavily hereditarily determined.

The above condensed survey may suffice to show that the role of MAVA findings in both forming and testing theoretical developments is considerable, especially in view of the pro-

longed dearth of evidence in this field and the previous impossibility of formulating results in terms of within- and between-family hereditary and environmental effects, and the correlations among them.

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MOTIVATION AND CUE UTILIZATION IN INTENTIONAL AND INCIDENTAL LEARNING[1]

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In a recent theoretical article, Easterbrook (1959) has attempted to relate level of motivation to the utilization of irrelevant cues in a wide range of behavioral situations. One of these situations pertains to the familiar incidental learning paradigm in which subject (*S*) is simultaneously exposed to relevant and irrelevant cues. Since this procedure provides no orienting instructions to ensure exposure to the irrelevant cues, cognizance of irrelevant cues would appear to be dependent upon the range or span of perception operating during performance on the intentional task (relevant cues). In general, as the range of perception extends beyond the central relevant cues, more peripheral cues are utilized, thus permitting more irrelevant or incidental cue learning; conversely, as the perceptual range constricts, fewer peripheral cues are utilized and incidental cue learning is reduced. According to Easterbrook, drive increments produce a restriction in range of cue use. This restriction, he clearly points out, may facilitate or impair the learning of relevant cues, depending upon the complexity of these cues, but will always impair the learning of peripheral irrelevant cues. As noted by Easterbrook, a similar position has been taken by Bruner, Matter, and Papanek (1955) relating what they call "breadth" of learning to both overmotivation and overlearning.

The present article is concerned further with motivation as a variable in incidental learning. More specifically, it is intended to be an evaluation and an extension of the position taken by Easterbrook that increasing drive level during intentional learning reduces the efficiency of incidental learning through the resulting decrease in irrelevant cue utilization.

However, before proceeding into the crux of the problem, some comments will be made on methodological procedures in incidental-learning experiments which should help serve to define the scope and limitations of the proposed extension.

Methodologies in Incidental Learning Experiments

In experimental studies investigating the principles of learning as they apply to incidental versus intentional learning, two basic methodologies have evolved. The first methodology requires a group of *S*s to learn a specified task incidentally and, usually, a second group of *S*s to learn the same task intentionally. That is, the intentional group learns the task with specific instructions to learn; the incidental group, on the other hand, is directed toward the task by some orienting instructions but is not instructed to learn the task. The important and critical condition in this methodology is that *S*s in the incidental group are given orienting instructions that assure perception of the learning material without an overt set to learn. Learning of the incidental material is then assumed to indicate the arousal of a covert or generalized set to learn. This methodology has been the pre-

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dominant one employed in the large number of recent investigations of incidental learning (e.g., Brown, 1954; Postman & Adams, 1956a, 1956b, 1957, 1958; Postman, Adams, & Bohm, 1956; Postman, Adams, & Phillips, 1955; Postman & Phillips, 1954; Saltzman, 1953, 1956, 1957). Studies in this methodological framework typically manipulate systematically either task, procedural, or orienting variables in an attempt to clarify the nature of the processes involved in incidental learning. Rarely, however, is motivation systematically manipulated within this framework. An important exception is a study by Spielberger, Goodstein, and Dahlstrom (1958) in which level of anxiety was investigated as a variable in incidental learning. In this study, however, only an incidental group was used without a corresponding group given instructions to learn.

In the second methodology, each group of *S*s employed in the experiment learns both an intentional task with instructions to learn and a different incidental task without instructions to learn. In other words, in this design, *S* serves as his own control for both intentional and incidental learning. This methodology lends itself more readily to investigating the effects of motivational variables on both intentional and incidental learning than does the first methodology. It is apparent that the restricting influence of motivation formulated by Easterbrook is applicable mainly to this methodology. In fact, four of the five studies cited by Easterbrook (Bahrick, 1954; Kohn, 1954; Silverman, 1954; Silverman & Blitz, 1956) as providing evidence for the effects of motivational concentration on irrelevant cue utilization in human learning are within this framework. The discussion that follows will therefore be restricted primarily to this methodology.

THE NATURE OF MOTIVATION AS A VARIABLE IN CUE UTILIZATION

In his treatment of motivation as a variable in cue utilization, Easterbrook (1959) is referring specifically to emotionally based drives. He states:

> it is an empirically derived generalization that, when the direction of behavior is constant, increase in drive is associated with a reduction in the range of cue use. This paper is devoted to the expansion and exploration of this generalization (pp. 183–184).

He further states:

> The term drive . . . refers to a dimension of emotional arousal or general covert excitement, the innate response to a state of biological deprivation or noxious stimulation, which underlies or occurs simultaneously with overt action and affects its strength and course. This emotional arousal is greater in neurotic than in normal subjects, greater than usual in subjects under stress or threat or in frustration, and in general greater in animals that have been "motivated" by any of the usual deprivations, noxious stimulations, or other incentives than it is in unmotivated or resting animals of the same species (p. 184).

From these statements it seems apparent that Easterbrook is attempting to relate *generalized drive* (*D*), as conceptualized by the Iowa group, to cue utilization.

One of the studies cited by Easterbrook as evidence for motivational restriction, in human learning, however, clearly does not attempt to manipulate generalized *D*. This is the study by Bahrick (1954) in which motivation is heightened by offering financial rewards as an incentive in the high motivational condition. Moreover, in relating motivation to perceptual-motor performance, Easterbrook considers in detail a study by Bahrick, Fitts, and Rankin (1952) in which motivation was again manipulated by financial incentives. This tendency on Easterbrook's part to equate motivational states aroused by specific sets with gen-

eralized *D* seems open to serious criticism. It is on this point that the thinking of the present writers begins to differ.

That the nature of the motivation operating during performance on the intentional task may be an important determiner of irrelevant cue utilization is evident from a recent study by Kausler, Trapp, and Brewer (1959) in which they duplicated the intentional and incidental tasks used by Bahrick (1954) with groups motivated by a generalized *D*, anxiety. In the Bahrick situation, motivation was clearly manipulated by an incentive-oriented set. He found that a high incentive group learned a serial list of geometric forms (relevant cues) more efficiently than a low incentive group. However, the high incentive group displayed significantly less incidental learning (irrelevant cues) than the low incentive group in the learning of colors filling in the geometric forms. These results are interpreted by Easterbrook as indicating a restriction or funneling of cue utilization in the high motivational group. The study of Kausler, Trapp, and Brewer (1959), however, yielded conflicting results. In this study the drive condition was generalized emotional *D* rather than incentive-oriented set. Two separate experiments were performed. In the first, anxiety was defined by score on the Taylor MAS; in the second, anxiety was induced by intense ego involving instructions. In both experiments, as in the Bahrick study, the high drive group did significantly better on the intentional task than the low drive group, but, unlike the Bahrick study, no difference was found between the two groups on the incidental task.

In comparing the Bahrick (1954) and Kausler et al. (1959) studies, the differential feature was in the nature of the motivation employed, incentive-oriented set versus generalized *D*. Farber (1955) has emphasized that nongeneralized motives have directive aspects but do not have dynamogenic properties. He states:

> The supposition that motivational variables have steering (associative) properties as well as energizing and reinforcing (drive) properties does not, of course, imply that the two kinds of properties are identical. Indeed, the virtue of the foregoing analyses of the concept of motivation lies in their separation of these functions. The associative function of a motive or any other variable is identified in terms of its tendency to elicit or facilitate a limited class of responses only. The drive function of a variable is demonstrated if: (a) its presence energizes or intensifies indiscriminately all reaction tendencies existing in a given situation; and/or (b) its elimination or reduction in magnitude is reinforcing, i.e., leads to the increased probability of recurrence, in the same situation, of the responses preceding its modification. Motives, it is presumed, have both functions, and if a variable clearly does not have both, its status as a motive is questionable (pp. 311–312).

Thus, incentive-oriented sets would seem to possess associative properties without drive properties. That is, they elicit particular responses that are goal directed but do not energize other responses that are irrelevant to the goal. In general, sets of this nature are experimentally induced by encouraging efficient performance on the relevant task without intentionally arousing emotional responses. This is typically accomplished by means of rewards or by task-oriented instructions. Generalized *D*, on the other hand, serves an energizing function in that its presence intensifies indiscriminately all reaction tendencies existing in a given situation, both relevant and irrelevant to the central task. In general, *D* is experimentally manipulated by the intentional arousal of emotional responses during the learning process. This is typically accomplished by differentiating between *S*s on some measure of intrinsic anxiety or by inducing anxiety through the use

of ego involving instructions, failure, stress, etc.

It is conceivable that in both the Bahrick and Kausler et al. studies increments in motivation, either associative or drive, resulted in a reduction of habit strength for the irrelevant cues through the restriction of perceptual range proposed by Easterbrook. Thus, the high motivational groups acquired less irrelevant habit strength than the low motivational group. In the Bahrick study this difference in habit strength is manifested because the difference in motivation between the two groups is relevant only to the intentional task and, therefore, does not enter into the multiplicative relationship (cf. Spence, 1958a, 1958b) between irrelevant habit strength and drive level. In the Kausler et al. study the generalized *D* would be expected to intensify the relatively weaker habit strength in the high drive group, thus bringing its response potential to a point comparable to that in the low drive group where habit strength is stronger but drive level is weaker. Hence, the groups would not differ in incidental learning.

POSITIONING OF RELEVANT AND IRRELEVANT CUES

The results of the Kausler et al. study also conflict with the studies by Silverman (1954) and Silverman and Blitz (1956) cited by Easterbrook as yielding supporting evidence for the restrictive influence of high motivation. In all three studies motivation was manipulated by an emotionally based generalized *D*. In the Silverman study anxiety was induced by threat of shock, and in the Kausler et al. study it was induced by ego involving instructions. In both the Silverman and Blitz and Kausler et al. studies intrinsic anxiety was employed by selecting extreme scorers on the MAS.

In both Silverman studies an increase in anxiety level was followed by the expected decrease in incidental learning in constrast to the no difference reported in the Kausler et al. study. Although the drive state does not differ in the three studies, there is another important differential between them. This is in terms of the relationship of the irrelevant cues to the relevant cues within the perceptual field. In the Kausler et al. study, as in the Bahrick study, the irrelevant cues are embodied within the relevant cues (colors within geometric forms), thus assuring close proximity, both spatially and temporally, between relevant and irrelevant cues. Consequently, even though anxious *S*s may have a restricted perceptual range, the irrelevant cues may well fall within that range. In contrast, the relevant and irrelevant cues in the Silverman studies are widely spaced within the perceptual field. In fact, in the Silverman (1954) study, the relevant cues are perceived by a different sense modality (vision) than are the irrelevant cues (audition). In the Silverman and Blitz (1956) study, the irrelevant cues (two-digit numbers) occupied a different section of a memory drum opening than did the relevant cues (nonsense syllables), and were separated from the relevant cues by six centimeters. It is very possible, therefore, that in the Kausler et al. study the differential perception between high and low anxious groups was not as pronounced as in the Silverman studies.

It is conceivable then that the amount of incidental learning under high and low motivation is affected differentially by position of the irrelevant cues within the perceptual field. That is, there is an interaction effect between level of motivation and cue positioning. In general, as the irrelevant cues are moved toward the periphery of the perceptual field, incidental learning will de-

crease, but the reduction will be greater for the high motivational levels than for low. Considerable systematic investigation is needed to establish the validity of this proposed hypothesis.

TASK AND PROCEDURAL VARIABLES

Other important variables to take into account in evaluating the effects of increased motivation on intentional and incidental learning are variables relating to the nature of the intentional task and method of presenting the intentional task. Particularly important variables would seem to be variables relating to the difficulty of the intentional task, such as, length, intratask competition, rate of presentation, etc. A number of studies employing the first methodology discussed earlier have systematically investigated these variables and demonstrated their importance in incidental learning studies (e.g., Neimark & Saltzman, 1953; Postman & Adams, 1957; Postman, Adams, & Phillips, 1955).

Differences in the tasks used, both intentional and incidental, make it more arduous to compare the results of studies with the simultaneous relevant-irrelevant cue methodology. This is especially true when motivation is varied qualitatively from study to study. Kausler and Trapp (1959) have suggested that studies making cross-drive comparisons should use, whenever possible, the same task in order to control for task effects.

Some evidence that the efficiency of incidental learning under different levels of motivation may be related to task difficulty is indicated in the study of Spielberger, Goodstein, and Dahlstrom (1958). Using the first methodology but without an intentional group, they found that the relationship between anxiety level (as defined by score on the MAS) and incidental learning (learning the designs on the Bender-Gestalt test) was a function of the difficulty of the learning material. On the easy designs the high anxious *S*s performed at a higher level than the low anxious *S*s; on the difficult designs the low anxious *S*s performed at a higher level than the high anxious *S*s.

The present writers believe that task difficulty variables operate in the intentional-incidental learning situation by altering the degree of concentration required of *S*s. Rate of presentation, for example, would be important in that the faster the rate, the greater the degree of concentration demanded. It is here suggested that intentional task variables which alter the degree of concentration required of *S* will affect further the range of attention for simultaneously present irrelevant ones. However, the net effect on the learning of irrelevant cue habits should also be a function of the nature of the motivation present (incentive-oriented versus generalized *D*), the level of motivation, and the positioning of irrelevant cues within the perceptual field. Thus studies relating degree of motivation to irrelevant cue utilization should also take into account the nature of the drive, the nature of the intentional task, and the positioning of the irrelevant cues.

For example, on easy tasks which require little concentration, high generalized drive *S*s should display more incidental learning than low drive *S*s (as in the Spielberger et al., 1958, study). The perceptual range should be fairly broad under these conditions and should incorporate irrelevant cues within close proximity to the relevant cues. Since generalized *D* reacts multiplicatively with irrelevant habit strength, the reaction tendency for high drive *S*s should be greater than for low drive *S*s. On difficult tasks, however, the perceptual range for irrelevant cues should be much narrower. Consequently, when

combined with the further restricting effect of high drive, the attention for these cues and subsequent habit strength should be considerably lower for high drive *S*s than for low drive *S*s. Under these conditions, low drive *S*s should begin to display superior incidental learning.

With incentive-oriented motivation, however, low motivation might be expected to result in superior incidental learning regardless of intentional task difficulty. This is likely because the motivation involved does not enter into a multiplicative relationship with habit strength. Nevertheless, the difference in incidental learning proficiency between high and low incentive groups could be expected to diminish as the intentional task decreases in difficulty. That is, the highly motivated *S*s would need to concentrate less on easy tasks, thus reducing the funneling effect on their attention to irrelevant cues; conversely, on difficult tasks requiring intense concentration, the funneling effect of high incentive motivation should be most pronounced.

It is obvious that considerable systematic research is needed, manipulating task variables along with motivational and positional variables, before the overall effects of level of motivation on irrelevant cue utilization can be determined. It does seem clear, however, that the phenomena Easterbrook seeks to embrace are too complex to be unified under one general principle, appealing as the thought may be. Even at this stage, the empirical data demand some extensions to his formulations. The elaborations presented here do reconcile existing conflicting findings. How well they can handle derivable hypotheses, like those suggested in the foregoing, remains to be seen. Studies to that effect are currently in process in our laboratories.

SUMMARY

Easterbrook has proposed that increasing motivation (specifically defined as emotional *D*) results in a funneling or focusing of attention to relevant cues when *S*s are simultaneously exposed to relevant and irrelevant cues. The present article suggests that the net effects of increasing motivation on cue utilization during incidental learning are functions of additional variables as well. Of particular importance seem to be the nature of the motivation (generalized *D* versus incentive-oriented set), position of the irrelevant cues relative to the relevant cues (central versus peripheral), and difficulty of the intentional task. This extension of Easterbrook's framework brings on a rapprochement with existing research findings and opens the way to more systematic investigations into an admittedly complex area.

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BLIND VARIATION AND SELECTIVE RETENTION IN CREATIVE THOUGHT AS IN OTHER KNOWLEDGE PROCESSES[1]

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This paper proposes to examine creative thought within the framework of a comparative psychology of knowledge processes, and in particular with regard to one theme recurrent in most knowledge processes. This theme may be expressed as follows:

1. A blind-variation-and-selective-retention process is fundamental to all inductive achievements, to all genuine increases in knowledge, to all increases in fit of system to environment.

2. The many processes which shortcut a more full blind-variation-and-selective-retention process are in themselves inductive achievements, containing wisdom about the environment achieved originally by blind variation and selective retention.

3. In addition, such shortcut processes contain in their own operation a blind-variation-and-selective-retention process at some level, substituting for overt locomotor exploration or the life-and-death winnowing of organic evolution.

Between a modern experimental physicist and some virus-type ancestor there has been a tremendous gain in knowledge[2] about the environment. In bulk, this has represented cumulated inductive achievements, stage by stage expansions of knowledge beyond what could have been deductively derived from what had been previously known. It has represented repeated "breakouts" from the limits of available wisdom, for if such expansions had represented only wise anticipations, they would have been exploiting full or partial knowledge already achieved. Instead,

[1] A partially overlapping version of this paper was presented at the Inter-Disciplinary Conference on Self-Organizing Systems, sponsored by the Office of Naval Research and the Armour Research Foundation of the Illinois Institute of Technology, Chicago, May 5–6, 1959. The proceedings of the conference are to be published by Pergamon Press under the title *Self-Organizing Systems*. The author is indebted to Carl P. Duncan for contributing to the development of many of the points involved.

[2] This extended usage of "knowledge" is a part of an effort to put "the problem of knowledge" into a behavioristic framework which takes full cognizance of man's status as a biological product of an evolutionary development from a highly limited background, with no "direct" dispensations of knowledge being added at any point in the family tree. The bibliographical citation of the several sources converging on this approach to the problem of knowledge, and the discussion of its relation to traditional philosophical issues and to the strategy of science are presented elsewhere (Campbell, 1959). Suffice it to say here that the position limits one to "an epistemology of the other one." The "primitives" of knowledge can not be sought in "raw feels" or in "phenomenal givens," or in any "incorrigible" elements. While man's conscious knowledge processes are recognized as more complex and subtle than those of lower organisms, they are not taken as more fundamental or primitive. In this perspective, any process providing a stored program for organismic adaptation in external environments is included as a knowledge process, and any gain in the adequacy of such a program is regarded as a gain in knowledge. If the reader prefers, he can understand the paper adequately regarding the term "knowledge" as metaphorical when applied to the lower levels in the developmental hierarchy. But since the problem of knowledge has resisted any generally accepted solution when defined in terms of the conscious contents of the philosopher himself, little seems lost and possibly something gained by thus extending the range of processes considered.

real gains must have been the products of explorations going beyond the limits of foresight or prescience, and in this sense blind. In the instances of such real gains, the successful explorations were in origin as blind as those which failed. The difference between the successful and unsuccessful was due to the nature of the environment encountered, representing discovered wisdom about that environment.

The general model for such inductive gains is that underlying both trial-and-error problem solving and natural selection in evolution, the analogy between which has been noted by several persons (e.g., Ashby, 1952; Baldwin, 1900; Pringle, 1951). Three conditions are necessary: a mechanism for introducing variation, a consistent selection process, and a mechanism for preserving and reproducing the selected variations. In what follows we shall look for these three ingredients at a variety of levels. But first a comment on the use of the word "blind" rather than the more usual "random." It seems likely that Ashby (1952) unnecessarily limited the generality of his mechanism in Homeostat by an effort to fully represent all of the modern connotations of random. Equiprobability is not needed, and is definitely lacking in the mutations which lay the variation base for organic evolution. Statistical independence between one variation and the next, while frequently desirable, can also be spared: in particular, for the generalizations essayed here, certain processes involving systematic sweep scanning are recognized as blind, insofar as variations are produced without prior knowledge of which ones, if any, will furnish a selectworthy encounter. An essential connotation of blind is that the variations emitted be independent of the environmental conditions of the occasion of their occurrence. A second important connotation is that the occurrence of trials individually be uncorrelated with the solution, in that specific correct trials are no more likely to occur at any one point in a series of trials than another, nor than specific incorrect trials. (Insofar as observation shows this not to be so, the system is making use of already achieved knowledge, perhaps of a general sort. The prepotent responses of an animal in a new puzzle box toward the apparent openings may thus represent prior general knowledge, transferred from previous learning or inherited as a product of the mutation and selective survival process.) A third essential connotation of blind is rejection of the notion that a variation subsequent to an incorrect trial is a "correction" of the previous trial or makes use of the direction of error of the previous one. (Insofar as mechanisms do seem to operate in this fashion, there must be operating a substitute process carrying on the blind search at another level, feedback circuits selecting "partially" adequate variations, providing information to the effect that "you're getting warm," etc.)

REVIEW OF THE THEME IN LOWER KNOWLEDGE PROCESSES

In this perspective, the epistemologically most fundamental knowledge processes are embodied in those several inventions making possible organic evolution. At the already advanced level of cellular life, this is a "learning" on the part of the species by the blind variation and selective survival of mutant individuals. In terms of the three requirements, variation is provided by the mutations, selection by the somewhat consistent or "knowable" vagaries of the environment, and preservation and duplication by the complex and rigid order of chromosome mitosis. Bisexuality, heterozygosity, and meiotic cell division represent a

secondary invention increasing the efficiency of the process through increasing the range of variation and the rate of readjustment to novel environments. The selection and preservation processes remain the same. The ubiquity of bisexuality, its several independent inventions, and the multifarious elaboration of the theme, all speak to its tremendous usefulness.

The higher evolutionary developments shift a part of the locus of adaptation away from a trial and error of whole organisms or gene pools, over to processes occurring within the single organism. Such processes are numerous, each being not only a device for obtaining knowledge, but also representing general wisdom about environmental contingencies already achieved through organic evolution, making possible more efficient achievement of detailed local knowledge. One of the most primitive of these is exploratory locomotion, described in the protozoa by Jennings (1906) and accepted as a model for Homeostat by Ashby (1952). Forward locomotion persists until blocked, at which point direction of locomotion is varied blindly until unblocked forward locomotion is again possible. The external physical environment is the selection agency, the preservation of discovery is embodied in the perseveration of the unblocked forward movement. At this level, the species has "discovered" that the environment is discontinuous, consisting of penetrable regions and impenetrable ones, and that impenetrability is to some extent a stable characteristic—it has discovered that when blocked it is a better strategy to try to go around than to wait until one can move through.

Insofar as individual organisms without distance receptors (such as paramecia and earthworms) can learn through contiguity, the species has already achieved the more general knowledge that there is some event-contingency stability in the environment. That is, in the degree to which individual learning is useful, there has been the species-level discovery of slower transformation processes on the part of relevant segments of the environment than of the organism. In addition, whereas the ultimate selection is life or death in encounters with the external environment, by the evolutionary stage at which learning is possible, much of this once-external criterion has been internalized. Crude environmental contingencies with low selection ratios are now represented as pleasures or pains, or as reinforcers more generally. The selection becomes much more sharp, but the contact with the environmental realities less direct.

The presence of a fundamental trial-and-error process in individual learning needs no elaboration or defense. Suffice it to say that recognition of such a process is found in all learning theories which make any pretense of completeness, including at least three of Gestalt inspiration (Campbell, 1956a). While higher vertebrate (and higher cephalopod) learning makes far more use of the short circuiting of overt trial and error by vision than is allowed for by the usual learning theory (Campbell, 1956b), for convenience here the multiplication of levels will be avoided by treating trial-and-error learning as a single process level.

The next and most striking class of discoveries are those centering around echo-location and vision. Woodworth (1921) has emphasized the achievement of a percept from the ingredients of sensation through a series of "trial-and-error perceptions." Thurstone (1924) has interpreted perception as a trial and error of potential locomo-

tions placed in a hierarchy of trial-and-error processes including both overt trial and error and ideational trial and error, in a book containing many anticipations of cybernetic concepts. Pumphrey (1950) interprets the primitive sense receptor of the fishes called the "lateral-line organ" as a crude echo-location device, making use of the reflected pulses of the fish's own swimming. Griffin (1958) has documented in detail the use by bats and cave birds of sonic and supersonic vocalizations selectively reflected by obstacles of the environment. Kellogg (1958) has made a similar case for the porpoise. Here is a powerful substitute for blind locomotor exploration. (See Simon, 1957, p. 264, for an estimate of such gains.) In echo location a wave pulse is emitted blindly in all directions. The obstacles of the environment selectively reflect the pulse from certain of these directions, and thus provide a feedback which is substitutable for that which would have been received had the animal locomoted in those directions. Radar guidance systems employ an analogous substitution of a blindly scanning electromagnetic wave pulse, in economical substitution for a blind scanning of the same environment of potential locomotions by full ship or projectile movements.

Visual perception seems interpretable as a substitute search process of similar order (Campbell, 1956b). The full analogy is weakened by the absence of an emitting process on the part of the organism. Instead, advantage is taken of diffuse electromagnetic waves made available from external sources. Consider first a pseudoeye consisting of but a single photoreceptor cell. (Such a device has been distributed for use by the blind in which a photocell output is transformed into a sound of variable pitch.) With such a device, blind scanning as in a radar system is essential. Brightness contours can be located and fixated by continual crossing, as in the "hunting" process in a mechanical servosystem, or as in the vocal pitch control in which a steady note is "held" only by a continuous search oscillation (Deutsch & Clarkson, 1959). To conceive of such an "eye" as a blind searching device substituting for a more costly blind locomotion in the explored directions is not difficult. The eyes of insects and vertebrates and the higher cephalopods differ from such a device by having multiple photocells, making possible selective reflection from objects in multiple directions at once. Each receptor cell can be conceived of as exploring the possibilities of locomotion in a given direction, the retina collectively thus exploring the possibilities of locomotion in a wide segment of potential directions for locomotion. Except as the eye is aimed by other sources of knowledge, these possibilities have been made "blindly" available without prescience or insight. For the "blindness" of an eyeless animal there has been substituted a process so efficient that we use it naively as a model for direct, unmediated knowing. But the process is still one of blind search and selective retention, in the sense employed in this paper.

Vision is a very complex and marvelous mechanism, and the brief presentation here does not do justice even to the random search components involved. Hebb (1949) has well documented the active search of eye movements, correcting the model of the inactive fixed-focus eye which is implicit in both Gestalt psychology and conditioning theory. Riggs (Riggs, Armington, & Ratliff, 1954) and Ditchburn (1955) have documented the essential role of the continuous low amplitude scanning provided by "phys-

iological nystagmus" or "fixation tremor." Platt (1958) has provided a brilliant analysis of the role of a blind "rubbing" process, his "lens-grinding" model, for the achievement of visual acuity and spatial representation in a visual system containing unaddressed elements. These and other considerations convince the present writer that although vision represents the strongest challenge to the generality a blind-variation-and-selective-retention aspect to all knowledge processes, it is not in fact an exception. These brief comments have not fully justified this conclusion, however.

Taking these echo location and visual exploratory processes collectively, several general aspects can be noted: all exploit a specific and limited coincidence, i.e., that objects impenetrable by organismic locomotion also are opaque to, or reflect, certain wave forms in the acoustical frequencies and in the bands of electromagnetic waves of the visual and radar spectra. It is this coincidence, unpredictable upon the basis of the prior knowledge available to the more primitive organisms, which makes possible such marvelously efficient shortcuts. Thus while phenomenologically vision is more direct than other knowledge processes, it is seen in this perspective as an indirect, substitute process. As in all substitute knowledge processes, the effectiveness is limited by the accuracy of the coding process, i.e., the translation terms between one level and another. Such coding is never exhaustive (Platt, 1956). It always involves abstraction, and along with this some fringe imperfection and proneness to systematic error. It must finally be checked out and corrected by overt locomotion. Its efficacy is limited by the *relevance* of the coding to the more fundamental level of behavior for which it is a substitute. This relevance was itself initially tested out by a blind-variation-and-selective-retention process at the level of organic evolution or early childhood learning. (Species differ in this regard.) The phenomenal directness of vision tempts us to make vision prototypic for knowing at all levels, and leads to that chronic belief in the potential existence of direct and "insightful" mental processes, a belief which it is one purpose of this paper to deny.

CREATIVE THOUGHT

Creative thought provides the next level knowledge process for the present discussion. At this level there is a *substitute* exploration of a *substitute* representation of the environment, the "solution" being selected from the multifarious exploratory thought trials according to a criterion which is in itself *substituting* for an external state of affairs. Insofar as the three substitutions are accurate, the solutions when put into overt locomotion are adaptive, leading to intelligent behavior which lacks overt blind floundering, and is thus a knowledge process. To include this process in the general plan of blind-variation-and-selective-retention, it must be emphasized that insofar as thought achieves innovation, the internal emitting of thought trials one by one is blind, lacking prescience or foresight. The process *as a whole* of course provides "foresight" for the overt level of behavior, once the process has blindly stumbled into a thought trial that "fits" the selection criterion, accompanied by the "something clicked," "Eureka," or "aha-erlebnis" that usually marks the successful termination of the process.

Today, we find the blind-variation-and-selective-retention model most plausibly applied at the levels of organic evolution and trial-and-error learning of animals, and least palatable as a

description of creative thinking. Historically, however, the phrase "trial and error" was first used to describe thinking, by Alexander Bain as early as 1855, two years before Darwin's publication of the doctrine of natural selection. Not only for historical interest, but also to further develop the psychology of creativity, the following quotations from him (Bain, 1874) are provided:

> Possessing thus the material of the construction and a clear sense of the fitness or unfitness of each new tentative, the operator proceeds to ply the third requisite of constructiveness—trial and error— . . . to attain the desired result. . . . The number of trials necessary to arrive at a new construction is commonly so great that without something of an affection or fascination for the subject one grows weary of the task. This is the *emotional* condition of originality of mind in any department (p. 593).
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> In the process of Deduction . . . the same constructive process has often to be introduced. The mind being prepared beforehand with the principles most likely for the purpose . . . incubates in patient thought over the problem, trying and rejecting, until at last the proper elements come together in the view, and fall into their places in a fitting combination (p. 594).
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> With reference to originality in all departments, whether science, practice, or fine art, there is a point of character that deserves notice. . . . I mean an Active turn, or a profuseness of energy, put forth in trials of all kinds on the chance of making lucky hits . . . Nothing less than a fanaticism of experimentation could have given birth to some of our grandest practical combinations. The great discovery of Daguerre, for example, could not have been regularly worked out by any systematic and orderly research; there was no way but to stumble upon it. . . . The discovery is unaccountable, until we learn that the author . . . got deeply involved in trials and operations far removed from the beaten paths of inquiry (p. 595).

In 1881 Paul Souriau presented a still more preponderant emphasis on the factor of chance as the sole source of true innovation. He asserts again and again that "le principe de l'invention est le hasard." In the main, he presents his argument through illustration and through the elimination of rival hypotheses about the inventive process, including deduction, induction, and "la methode." A positive explanation of the process is hard to find. This sample will illustrate his approach:

> A problem is posed for which we must invent a solution. We know the conditions to be met by the sought idea; but we do not know what series of ideas will lead us there. In other words, we know how the series of our thoughts must end, but not how it should begin. In this case it is evident that there is no way to begin except at random. Our mind takes up the first path that it finds open before it, perceives that it is a false route, retraces its steps and takes another direction. Perhaps it will arrive immediately at the sought idea, perhaps it will arrive very belatedly: it is entirely impossible to know in advance. In these conditions we are reduced to dependence upon chance.
>
> In the case just analysed we supposed that we had to solve a problem already stated for us. But how was the problem-statement itself found? It is said that a question well posed is half answered. If so, then true invention consists in the posing of questions. There is something mechanical, so to speak, in the art of finding solutions. The truly original mind is that which discovers problems. But here again, it does no good to speak of method, since method is the application of already existing discoveries. The discovery of a new problem can therefore only be fortuitous. Thus we see the role of logic diminish and that of chance increase as we approach closer to true invention. Chance is the first principle of invention: it is what has produced method, nourished it, and made it fertile. Method can only analyse the ideas which come to it from elsewhere, drawing out their consequences and exhausting their contents. Left to itself method soon becomes sterile. Methodological minds cannot help having a feeling of disdain for adventurous minds which affirm before proving and believe before knowing. But they must recognize that without such audacity, no progress would be possible. The mind is not able to revise itself upon its own

foundations. New ideas cannot have prototypes: their appearance can only be attributed to chance (pp. 17–18).

Souriau has not only the notion of chance combinations, but also the concept of their being produced in large numbers which are generally worthless and from which only the rare ones fitting a goal or criterion are selected. These two widely separated quotations illustrate this:

> By a kind of artificial selection, we can in addition substantially perfect our thought and make it more and more logical. Of all of the ideas which present themselves to our mind, we note only those which have some value and can be utilized in reasoning. For every single idea of a judicious and reasonable nature which offers itself to us, what hosts of frivilous, bizarre, and absurd ideas cross our mind. Those persons who, upon considering the marvelous results at which knowledge has arrived, cannot imagine that the human mind could achieve this by a simple fumbling, do not bear in mind the great number of scholars working at the same time on the same problem, and how much time even the smallest discovery costs them. Even genius has need of patience. It is after hours and years of meditation that the sought-after idea presents itself to the inventor. He does not succeed without going astray many times; and if he thinks himself to have succeeded without effort, it is only because the joy of having succeeded has made him forget all the fatigues, all of the false leads, all of the agonies, with which he has paid for his success (p. 43).
>
> . . . If his memory is strong enough to retain all of the amassed details, he evokes them in turn with such rapidity that they seem to appear simultaneously; he groups them by chance in all the possible ways; his ideas, thus shaken up and agitated in his mind, form numerous unstable aggregates which destroy themselves, and finish up by stopping on the most simple and solid combination (pp. 114–115).

The phrase "artificial selection" is reminiscent of Darwin's writings, although Souriau makes no mention of the selective-survival model of evolution, (nor does he cite the ideas of any other person whatsoever).[3] Note how similar the final quotation is to Ashby's (1952) phrasing of the inevitable self-elimination of unstable combinations:

> Just as, in the species, the truism that the dead cannot breed implies that there is a fundamental tendency for the successful to replace the unsuccessful, so in the nervous system does the truism that the unstable tends to destroy itself imply that there is a fundamental tendency for the stable to replace the unstable (p. vi).

Ernst Mach was another great 19th century thinker about thinking who emphasized this model. We today remember him most as a psychologist-physicist-philosopher who contributed to the present day positivistic recognition of the hypothetic character of our constructions of the world and who first made explicit the empirical presumptions involved in the physicist's assumption of an Euclidian space. But when, at the age of 57 in 1895, he was called back to his alma mater the University of Vienna to assume a newly created position of Professor of the History and Theory of Inductive Science, he chose a quite different theme for his inaugural address. His title was "on the part played by accident in invention and discovery." The occasion indicates the importance he gave to the message, and indeed,

[3] Souriau's presentation is in general quite modern in spirit, although associationistic in a way some would find dated, and vigorously deterministic in a way now undermined by subatomic physics, although not necessarily so for the problems of which he treats. He comments wisely on many topics not covered here, including simultaneous independent invention and the Zeitgeist, the social conditions of creativity and invention, the dissonance created by discrepant opinions of others, the congruence of free will and determinism, and both the conflict and interdependency between erudition and innovation. His attacks on both deduction and induction are reminiscent of Peirce's later critiques.

his paper is a neglected classic in the psychology of knowledge processes. These quotations (Mach, 1896) further reinforce the model of creative thought being presented:

> The disclosure of new provinces of facts before unknown, can only be brought about by accidental circumstances . . . (p. 168).
>
> In such [other] cases it is a psychical accident to which the person owes his discovery—a discovery which is here made "deductively" by means of mental copies of the world, instead of experimentally (p. 171).
>
> After the repeated survey of a field has afforded opportunity for the interposition of advantageous accidents, has rendered all the traits that suit with the word or the dominant thought more vivid, and has gradually relegated to the background all things that are inappropriate, making their future appearance impossible; then from the teeming, swelling host of fancies which a free and high-flown imagination calls forth, suddenly that particular form arises to the light which harmonizes perfectly with the ruling idea, mood, or design. Then it is that that which has resulted slowly as the result of a gradual selection, appears as if it were the outcome of a deliberate act of creation. Thus are to be explained the statements of Newton, Mozart, Richard Wagner, and others, when they say that thoughts, melodies, and harmonies had poured in upon them, and that they had simply retained the right ones (p. 174).

Poincaré (1908, 1913) in his famous essay on mathematical invention presents a point of view which is also judged to be in agreement. He first gives an example in imagery: "One evening, contrary to my custom, I drank black coffee and could not sleep. Ideas rose in crowds; I felt them collide until pairs interlocked, so to speak, making a stable combination" (Poincaré, 1913, p. 387). Poincaré feels that it is rare for this blind permuting process to rise into conscious awareness, and that as a rule only the successful selected alternatives enter consciousness. Because of the relevance of Poincaré's comments; because Hadamard (1945) has cited him in opposition to the accidentalist position while he is read here as favoring the selective-retention version of it; and because of all of the sources cited he would most generally be respected as truly creative (in the field of mathematics) these longish excerpts (Poincaré, 1913) are read into the record:

> It is certain that the combinations which present themselves to the mind in a sort of sudden illumination, after an unconscious working somewhat prolonged, are generally useful and fertile combinations, which seem the result of a first impression. Does it follow that the subliminal self, having divined by a delicate intuition that these combinations would be useful, has formed only these, or has it rather formed many others which were lacking in interest and have remained unconscious?
>
> In this . . . way of looking at it, all the combinations would be formed in consequence of the automatism of the subliminal self, but only the interesting ones would break into the domain of consciousness. And this is still very mysterious. What is the cause that, among the thousand products of our unconscious activity, some are called to pass the threshold, while others remain below? Is it a simple chance which confers this privilege? Evidently not; among all the stimuli of our senses, for example, only the most intense fix our attention, unless it has been drawn to them by other causes. More generally the privileged unconscious phenomena, those susceptible of becoming conscious, are those which, directly or indirectly, affect most profoundly our emotional sensibility (p. 391).
>
> . . . we reach the following conclusion: The useful combinations are precisely the most beautiful, I mean those best able to charm this special sensibility that all mathematicians know, but of which the profane are so ignorant as often to be tempted to smile at it.
>
> What happens then? Among the great numbers of combinations blindly formed by the subliminal self, almost all are without interest and without utility; but just for that reason they are also without effect upon the esthetic sensibility. Consciousness will never know them; only certain ones are harmonious, and, consequently, at once useful and beautiful. They will be capable of

touching this special sensibility of the geometer of which I have just spoken, and which, once aroused, will call our attention to them, and thus give them occasion to become conscious.

This is only a hypothesis, and yet here is an observation which may confirm it: when a sudden illumination seizes upon the mind of the mathematician, it usually happens that it does not deceive him, but it also sometimes happens, as I have said, that it does not stand the test of verification; well, we almost always notice that this false idea, had it been true, would have gratified our natural feeling for mathematical elegance.

Thus it is this special esthetic sensibility which plays the role of the delicate sieve of which I spoke, and that sufficiently explains why the one lacking it will never be a real creator.

Yet all the difficulties have not disappeared. The conscious self is narrowly limited, and as for the subliminal self we know not its limitations, and this is why we are not too reluctant in supposing that it has been able in a short time to make more different combinations than the whole life of a conscious being could encompass. Yet these limitations exist. Is it likely that it is able to form all the possible combinations, whose number would frighten the imagination? Nevertheless that would seem necessary, because if it produces only a small part of these combinations, and if it makes them at random, there would be small chance that the *good*, the one we should choose, would be found among them.

Perhaps we ought to seek the explanation in that preliminary period of conscious work which always precedes all fruitful unconscious labor. Permit me a rough comparison. Figure the future elements of our combinations as something like the hooked atoms of Epicurus. During the complete repose of the mind, these atoms are motionless, they are, so to speak, hooked to the wall; so this complete rest may be indefinitely prolonged without the atoms meeting, and consequently without any combination between them.

On the other hand, during a period of apparent rest and unconscious work, certain of them are detached from the wall and put in motion. They flash in every direction through the space (I was about say the room) where they are enclosed, as would, for example, a swarm of gnats or, if you prefer a more learned comparison, like the molecules of gas in the kinematic theory of gases. Then their mutual impacts may produce new combinations.

What is the role of the preliminary conscious work? It is evidently to mobilize certain of these atoms, to unhook them from the wall and put them in swing. We think we have done no good, because we have moved these elements a thousand different ways in seeking to assemble them, and have found no satisfactory aggregate. But, after this shaking up imposed upon them by our will, these atoms do not return to their primitive rest. They freely continue their dance.

Now, our will did not choose them at random; it pursued a perfectly determined aim. The mobilized atoms are therefore not any atoms whatsoever; they are those from which we might reasonably expect the desired solution. Then the mobilized atoms undergo impacts which make them enter into combinations among themselves or with other atoms at rest which they struck against in their course. Again I beg pardon, my comparison is very rough, but I scarcely know how otherwise to make my thought understood.

However it may be, the only combinations that have a chance of forming are those where at least one of the elements is one of those atoms freely chosen by our will. Now, it is evidently among these that is found what I call the *good combination*. Perhaps this is a way of lessening the paradoxical in the original hypothesis.

Another observation. It never happens that the unconscious work gives us the result of a somewhat long calculation *all made*, where we have only to apply fixed rules. We might think the wholly automatic subliminal self particularly apt for this sort of work, which is in a way exclusively mechanical. It seems that thinking in the evening upon the factors of a multiplication we might hope to find the product ready made upon our awakening, or again that an algebraic calculation, for example a verification, would be made unconsciously. Nothing of the sort, as observation proves. All one may hope from these inspirations, fruits of unconscious work, is a point of departure for such calculations. As for the calculations themselves, they must be made in the second period of conscious work, that which follows the inspiration, that in which one verifies the results of this inspiration and deduces their consequences. The rules of these calculations are strict and complicated. They require discipline, attention, will, and therefore consciousness. In the subliminal

self, on the contrary, reigns what I should call liberty, if we might give this name to the simple absence of discipline and to the disorder born of chance. Only, this disorder itself permits unexpected combinations (pp. 392–394).

In addition to these pioneers there of course have been numerous others who in some manner have made a substitute trial and error in a modeled or mnemonic environment an important aspect of their description of thinking. In rough chronology these include Baldwin (1906), Pillsbury (1910), Rignano (1923), Woodworth (1921), Woodworth & Schlosberg (1954), Thurstone (1924), Tolman (1926), Hull (1930), Muenzinger (1938), Miller and Dollard (1941), Craik (1943), Boring (1950), Humphrey (1951), Mowrer (1954), Sluckin (1954), and many others.

OBJECTIONS TO THE MODEL

The Gestalt Protest

The trial-and-error theme in learning was of course one part of the syndrome of ideas against which Gestalt psychology eloquently protested. In spite of this, there is judged to be no inherent conflict between the perspectives of this paper and the Gestalt position. To make this interpretation, it is necessary to regard neither traditional associationism nor Gestalt psychology as discrete integrated wholes, but instead to regard each as congeries of which the parts may be separately accepted or rejected.

The Gestaltists are judged to have validly rejected Thorndike's (1898) description of animal problem solving as solely a matter of overt locomotor trial and error. As this writer (Campbell, 1956b) has argued previously, recognizing vision as a substitute trial-and-error process leads to the expectation that some locomotor problems will be solved by this means, obviating overt trial and error. Even more so does the model for thought. To the present writer the Gestaltists were correct descriptively even though epistemologically the trial-and-error process remains fundamental to discovery. In Wertheimer's (1959) specific contrasts between insightful problem solving and blind trial and error, it is a trial and error of overt manipulation which is involved. Furthermore, as Humphrey (1951) and Woodworth and Schlosberg (1954) point out, the Gestalt descriptions of problem solving provide ample evidence of both fortuitous solutions and misleading "insights." The recurrent Gestaltist protest that even the errorful trials are "intelligent" and that the subsequent trials make use of what was learned through the error are taken here as equivalent to the statements that the problem solver had some valid general knowledge to begin with, and that before acting he employed the substitute trial and error of thought or vision.

The blind-variation-and-selective-retention model of thought joins the Gestaltists in protest against the picture of the learning organisms as a passive induction machine accumulating contingencies. Instead, an active generation and checking of thought-trials, hypotheses, or molar responses is envisaged. The model at the level of thought places essential importance upon internalized selective criteria against which the thought trials are checked. Poincaré's (1913) esthetic criteria and the Gestalt qualities of wholeness, symmetry, organized structure, and the like can be regarded as built-in selective criteria completely compatible with the model. Pringle (1951) for example, has proposed a selective-retention model of central nervous system action in which systematic temporal patterns provide the selective criteria in a resonance process.

Nor does the model here presented specify the nature of the thought trials employed. There must often be a trial and error among possible general principles, or among rational abstractions, or field reorganizations, or recenterings. Both the blind variation and selective survival model, and Gestalt theory emphasize the advantage of breaking out of old ruts, and the disadvantages of set and rote drill (Boring, 1950; Dunker, 1945; Katona, 1940; Luchins, 1942; Wertheimer, 1959). Furthermore, the encountering in thought of an idea which fits can be accompanied at the phenomenal level by a joyful "ahaerlebnis" or a Gestalt experience of "closure," and at the overt performance level by a sudden and stable improvement signifying "insight." There is no essential disagreement here. Nor is the trial-and-error model without phenomenological support. Note the highly similar testimony from the disparate historical citations provided above, especially in the imagery of multitudinous, loosened, agitated, teeming, colliding, and interlocking ideas.

This is not to say that a Gestalt psychologist would be happy with the blind-variation-and-selective-retention description of thought processes. Nor are all aspects of the Gestalt syndrome here accepted. While "insight" is accepted as a phenomenal counterpart of the successful completion of a perhaps unconscious blind-variation cycle, its status as an explanatory concept is rejected, especially as it connotes "direct" ways of knowing. Furthermore, when publicized as a part of an ideology of creativity, it can reduce creativity through giving students a feeling that they lack an important gift possessed by some others, a feeling which inhibits creative effort and increases dependence upon authority. Polya (1945, 1954) has described such an inhibiting tradition in the teaching of mathematics.

Individual Differences and Genius

Another prevalent orientation antithetical in spirit to the blind-variation-and-selective-retention model may be called the "mystique of the creative genius and the creative act." This is related to our deeply rooted tendency toward causal perception (e.g., Heider, 1944), a tendency to see marvelous achievements rooted in equally marvelous antecedents. It takes the form of the "fallacy of accident" and of "*post hoc ergo propter hoc.*" Let a dozen equally brilliant men each propose differing guesses about the unknown in an area of total ignorance, and let the guess of one man prove correct. From the blind-variation-and-selective-survival model this matching of guess and environment would provide us with new knowledge about the environment but would tell us nothing about the greater genius of the one man—he just happened to be standing where lightning struck. In such a case, however, we would ordinarily be tempted to look for a subtle and special talent on the part of this lucky man. However, for the genuinely unanticipatable creative act, our "awe" and "wonder" should be directed outward, at the external world thus revealed, rather than directed toward the antecedents of the discovery. Just as we do not impute special "foresight" to a successful mutant allele over an unsuccessful one, so in many cases of discovery, we should *not* expect marvelous consequents to have had equally marvelous antecedents. Similarly, in comparing the problem-solving efforts of any one person; from the selective survival model it will be futile, in the instance of a genuinely innovative achievement, to look for special antecedent conditions not obtaining for

blind-alley efforts: just insofar as there has been a genuine gain in knowledge, the difference between a hit and a miss lies in the selective conditions thus newly encountered, not in talent differences in the generation of the trials.

This is not to deny individual differences in creative intellect. Indeed, the blind-variation-and-selective-retention model of creative thought predicts such talent differences along all of the parameters of the process. This is to emphasize, however, that explanations in terms of special antecedents will very often be irrelevant, and that the causal-interpretative biases of our minds make us prone to such over-interpretations, to *post-hoc-ergo-proper-hoc* interpretations, deifying the creative genius to whom we impute a capacity for direct insight instead of mental flounderings and blind-alley entrances of the kind we are aware typify our own thought processes. Ernst Mach (1896) notes our nostalgia for the directly-knowing genius: "To our humiliation we learn that even the greatest men are born more for life than for science in the extent to which even they are indebted to accident" (p. 175).

What are the ways in which thinkers might be expected to differ, according to the trial-and-error model? First, they may differ in the accuracy and detail of their representations of the external world, of possible locomotions in it or manipulations of its elements, and of the selective criteria. Differences in this accuracy of representation correspond to differences in degree of information and intelligence. Second, thinkers can differ in the number and range of variations in thought trials produced. The more numerous and the more varied such trials, the greater the chance of success. Bain has emphasized the role of fanaticism or extreme dedication in producing large volumes of such explorations. Bain, Souriau, Mach, and Poincaré have all emphasized the role of advance preparation in assembling the elements whose blind permutation and combination make possible a wide range of trials. Many observers have emphasized the role of set and familiarity in reducing the range of variations, and have recommended ways of reducing trial-to-trial stereotypy, as by abandoning the problem for awhile, going on to other things. Devices abound which are designed to increase the likelihood that all permutations be considered and are used by most of us, as in going through the alphabet in finding rhymes or puzzle words. There are no doubt age differences in the rapidity and uninhibited range of thought-trial production. The sociology of knowledge makes an important contribution here: persons who have been uprooted from traditional cultures, or who have been thoroughly exposed to two or more cultures, seem to have the advantage in the range of hypotheses they are apt to consider, and through this means, in the frequency of creative innovation. Thorstein Veblen (1919) has espoused such a theory in his essay on the intellectual preeminence of Jews, as has Robert Park (1928) in writing of the role of "the marginal man" in cultural innovation. (See also Seeman, 1956.) And more generally, it is the principle of variation which leads us to expect among innovators those of personal eccentricity and bizarre behavior. We can also see in this principle the value of those laboratories whose social atmospheres allow wide ranging exploration with great tolerance for blind alley entrances.

The value of wide ranging variation in thought trials is of course vitiated if there is not the precise application of a selective criterion which weeds out the overwhelming bulk of inadequate

trials. This editing talent undoubtedly differs widely from person to person, as Poincaré (1908, 1913) has emphasized. With regard to selection criteria, one further point should be made. Much of creative thought is opportunistic in the sense of having a wide number of selective criteria available at all times, against which the thought trials are judged. The more creative thinker may be able to keep in mind more such criteria, and therefore increase his likelihood of achieving a serendipitous (Cannon, 1945; Merton, 1949) advance on a problem tangential to his initial main line of endeavor (e.g., Barber & Fox, 1958). Further areas of individual differences lie in the competence of the retention, cumulation, and transmisison of the encountered solutions.

It need not be expected that these dimensions of talent all go together. In organic evolution, the variation process of mutation and the preservation of gains through genetic rigidity are at odds, with an increase in either being at the expense of the other, and with some degree of compromise being optimum. Just so we might expect that a very pure measure of innovative range in thought and a very pure measure of rote memory might be even negatively correlated, as Saugstad (1952) seems to have found, and similarly for innovative range and selective precision. Such considerations suggest complementary combinations of talent in creative teams, although the uninhibited idea-man and the compulsive edit-and-record type are notoriously incompatible office mates.

Notice regarding the individual differences thus described that while they do make creative innovation much more likely on the part of some individuals than others, they do not place the joys of creative innovation beyond the reach of the less gifted. Indeed, looking at large populations of thinkers, the principles make it likely that many important contributions will come from the relatively untalented, undiligent, and uneducated, even though on an average contribution per capita basis, they will contribute much less, points which Souriau (1881) has noted. The intricacy of the tradition to which innovation is being added of course places limitations in this regard.

The Enormous Domain of Possible Thought-Trials to be Searched

A final type of objection to the blind-variation-and-selective-retention model of thought needs to be considered. This objection is to the effect that the domain of possible thought trials is so large that the solution of a given problem would take an impossibly long time were a search of all possibilities to be involved, either through a systematic scanning of all possibilities where these are enumerable, or through a random sampling of the universe of possibilities. Time and trial estimates thus based can be overwhelming, as Kurt Lasswitz's story "The Universal Library" (1958) dramatically illustrates. Other parodies of our model occur in literature as far back as Swift's portrait of the Academy of Lagado in *Gulliver's Travels* (1941, pp. 166–169). (Ley, 1958, traces such ideas back to Lully, ca. 1200.) Newell, Shaw, and Simon (1958a, 1958b) refer in this vein to what they call the "British Museum Algorithm," i.e., the possibility of a group of trained chimpanzees typing at random producing by chance in the course of a million years all of the books in the British Museum. Such parodies seem effectively to reject the blind-variation-and-selective-retention model through a *reductio ad absurdum*. Needless to say, such a

rejection is not accepted in the present paper. As a matter of fact, it is judged to be in the same class as parallel objections to the theory of natural selection in evolution. Similar features in these two instances make the accidentalist interpretation more acceptable.

1. Neither in organic evolution nor in thought are all problems solved, nor all possible excellent solutions achieved. There is no guarantee of omniscience. The knowledge we do encounter is achieved against terrific odds. (Those advocating heuristically-programed problem-solving computers are careful not to guarantee solutions, and this modesty should be extended to all models of creative thought.)

2. The tremendous number of nonproductive thought trials on the part of the total intellectual community must not be underestimated. Think of what a small proportion of thought becomes conscious, and of conscious thought what a small proportion gets uttered, what a still smaller fragment gets published, and what a small proportion of what is published is used by the next intellectual generation. There is a tremendous wastefulness, slowness, and rarity of achievement.

3. In biological evolution and in thought, the number of variations explored is greatly reduced by having *selective criteria imposed at every step*. Thus mutant variations on nonadaptive variations of the previous generation are never tested—even though many wonderful combinations may be missed therefore. Some of the "heuristics" currently employed in logic and chess playing machines (Newell, Shaw, & Simon, 1958a, 1958b) have the similar effect of evaluating all next-possible moves in terms of immediate criteria, and then of exploring further variations upon only those stems passing the screening of each prior stage. It is this strategy of cumulating selected outcomes from a blind variation, and then exploring further blind variations only for this highly select stem, that, as R. A. Fisher has pointed out (e.g., 1954, p. 91) makes the improbable inevitable in organic evolution. This strategy is unavoidable for organic evolution, but can obviously be relaxed in thought processes and in machine problem solving. However, the Pandora's box of permutations opened up by such relaxation can be used to infer that, in general, thought trials are selected or rejected within one or two removes of the established base from which they start. In constructing our "universal library" we stop work on any volume as soon as it is clear that it is gibberish.

4. When we make estimates of the number of permutations which would have to be culled to obtain a given outcome, we often assume that problem solving was undertaken with that one fixed goal in mind. This overlooks the opportunistic, serendipitous course of organic evolution and of much of creative thinking. The likelihood of a productive thought increases with the wider variety of reasons one has for judging a given outcome "interesting." To neglect this opportunistic multipurposedness gives one a poor base for estimating the probability of encountering the one outcome hit upon and recorded. Thus when Newell, Shaw, and Simon's "Logic Theorist" (1958a, 1958b) sets out to prove the 60-odd theorems in a given chapter of *Principia Mathematica*, it may face a more formidable task than did Whitehead and Russell in generating them, if, except for the dozen classic theorems reproduced, Whitehead and Russell were otherwise free to record every deduction they encountered which seemed "interesting"

or "nontrivial." Wigglesworth (1955) has noted this strategy on the part of "pure" scientists, in commenting on the relationship between pure and applied scientists in wartime:

> In the pure science to which they were accustomed, if they were unable to solve problem A they could turn to problem B, and while studying this with perhaps small prospect of success they might suddenly come across a clue to the solution of problem C (p. 34).

In presenting their case for adding "heuristics" to the program of the "Logic Theorist," Newell, Shaw, and Simon have emphasized the inadequacy of blind trial and error. So has Miller (1959) in advocating the heuristic of searching backward from the goal.[4] There is, however, no essential disagreement between their point of view and the one offered here. By adding heuristics mechanical thought processes have indeed been made more like those of human beings, both in adequacy and type of errors. Such innovations have obviated the protests of those such as Wisdom (1952) and Mays (1956) who, while conceding that machines could choose good moves at chess or solve logic problems, have found the machines failing to imitate life just in their orderly inspection of all possibilities. Newell, Shaw, and Simon recognize that a machine which would develop its own heuristics would have to do so by a trial and error of heuristic principles, with no guarantee that any would work. They further recognize that possession of an effective heuristic represents already achieved general knowledge about the domain under search, and that adding to this general knowledge will be a blind search process. (The devices of learning and vision and of coding environmental possibilities for thought-search

[4] Miller (1959, pp. 244–246) is wrong in implying that the strategy of working backward from the goal eliminates the necessity of symbolic trial and error in creative thinking. His mistake comes from assuming that only one path leads into a goal or subgoal. In the spatial locomotion problems from which his concrete illustrations come, and for the logic problems used by Newell, Shaw, and Simon, the paths into any position are not singular, but are instead typically as numerous as paths leading out. A *pure* strategy of working from the goal back to the start would thus involve exploring just as many permutations as the pure strategy of exploring all paths from the start position. However, there is a useful strategy available to symbolic trial and error and not to overt trial and error, in working concurrently from both ends. This produces a dramatic advantage in the number of permutations generated, and a smaller but still substantial gain in the number of comparisons. In the instance suggested by Miller in which each locus branches into 4 alternatives and in which the start and goal turn out to lie 7 stages apart, either of the pure strategies would generate $4 + 4^2 + 4^3 + 4^4 + 4^5 + 4^6 + 4^7$ or 21,844 permutations (neglecting the probable achievement of success before exhausting the 4^7 generation of alternatives). For the mixed approach, the junction would be encountered at the third stage away from each end, or when $2(4 + 4^2 + 4^3)$ or 168 alternatives had been examined. In the pure strategy, there would be 21,845 comparisons involved, that is, the start position and each subsequent alternative would be compared with the goal. In the mixed strategy, many more comparisons per alternative are required. Each permutation must be compared with each of the current and previous permutations on the other stem, which in this instance amounts to 7,225 comparisons. If the comparisons are regarded as equally costly as the generating and storing of alternatives, the savings of the mixed approach over either of the pure approaches would amount to approximately 1 to 6, a very handsome gain for any heuristic. This gain is larger as the number of branches at each stage increases. Advocacy of the heuristic of working backwards in what is essentially this mixed form is present in Polya (1945), Wisdom (1952), and Newell, Shaw, and Simon (1958a, 1958b). In none of these is it claimed that trial and error is eliminated, while all point to the reduction in trials which it can achieve.

all represent heuristics in this sense.) They might also agree that most heuristic devices will be limited to the specific domain of their discovery, and can only be extended to other domains on a trial basis. They would probably also agree that no problem solving process will be "direct." The disagreements I have with their excellent paper on the processes of creative thinking (Newell, Shaw, & Simon, 1958b) are thus minor matters of emphasis, but may be worth stating nonetheless, to further clarify the position here advocated. They say, for example:

> We have given enough estimates of the sizes of the spaces involved . . . to cast suspicion upon a theory of creativity which places its emphasis upon increase in trial and error (p. 63).

The blind-variation-and-selective-retention model unequivocally implies that *ceteris paribus*, the greater the heterogeneity and volume of trials the greater the chance of a productive innovation. Doubling the number of efforts very nearly doubles the chance of a hit, particularly when trials are a small part of the total domain and the repetitiousness among trials is low. But they too recognize unconventionality and no doubt numerosity as a necessary, if not a sufficient condition of creativity (1958b, p. 62). What they would validly stress, is the very frequent tactical advantage of a trial and error of general strategic principles over a trial and error involving no classificatory effort nor attempt to use clues, and, once such general heuristics have been discovered, the advantages of a hierarchized trial and error process. The advantage of such a strategy depends upon the ecology, of course, but we are in general justified in expecting solutions to be nonrandomly distributed, and to show significant contingencies with prior clues. Polya (1945, 1954) has, of course, been a major source of inspiration for all efforts to introduce heuristics into problem solving, and for him a trial and error approach is a heuristic of fundamental importance.

Another minor point of disagreement may be mentioned. In their efforts to consider how a "Logic Theorist" might be programed to learn a general heuristic from hindsight they propose that it keep a record of the outcomes of all past trials, successful and unsuccessful, in order to be able to scan its experience for general principles of strategy (1958b). Implementing this would put a tremendous strain upon memory storage, and would introduce a scanning process as time consuming as the original search process which produced the record. The strategy of organic evolution is to keep a record only of what works, even at the expense of repeating its errors. The general preponderance of wrong tries at every level, plus problems of memory glut and access, suggests a similar strategy for all knowledge processes. Heuristics can probably best be learned through a trial and error of heuristics, tried on new problem sets rather than old.

STATUS AS A THEORY

At the level here developed, one might better speak of an "orientation to," or a "perspective on" creative thought processes, rather than a "theory of." At many points, this perspective merely points to problems, rather than taking that step toward theory of providing guesses at answers: e.g., for the processes here outlined to be possible, theory must not only provide several memory processes, but most importantly, must specify a possible mechanism for the trial-and-error

search of these. From such specifications will come the subtlety of prediction characteristic of a developed theory.[8] While the perspective even in its very general state has some unequivocal empirical implications, the major advantage to it may be metaphysical, or at least metatheoretical. Like the theory of natural selection in organic evolution, it provides an understanding of marvelously purposive processes without the introduction of teleological metaphysics or of pseudocausal processes working backward in time.

Note that there are still ambiguities about the status *as theory* of the well established principle of organic evolution through natural selection, even though now buttressed with the detailed genetic model of the variation and retention processes. Scriven (1959) has called it an explanatory rather than a predictive theory. While the present writer feels that Scriven has somewhat undervalued the experimental studies of evolution with viruses, bacteria, and insects, and may have mistaken the past absence of experimental settings appropriate to testing the theory for an inherent attribute of the type of theory per se; he has called attention, nonetheless, to some serious problems. Even Sewall Wright (1960), whose statistical genetic theory of evolution has added subtle details to the overall description of the process, has commented in a similar vein:

> The theory is deterministic only in an exceedingly limited sense. It is essentially a theory of the conditions favorable for an ever continuing process that is essentially unpredictable in its details. There can be no formula for serendipity (p. 148).

The problems which a selective retention theory of creative thinking shares with that of evolution include the following:

1. The basic insight, so useful and so thrilling when first encountered, is close to being an analytic tautology rather than a synthetic description of process: if indeed variations occur which are differentially selected and propagated, then an evolutionary process toward better fit to any set of consistent selective criteria is inevitable.

2. For most applications of the selective retention model, the variation is taken as a descriptive given, as an unexplained part of the explanation. While other predictive theories likewise depend upon unexplained processes at a more molecular level, this instance may seem evasive at a particularly crucial point, and has indeed been taken as a denial of determinism or as a rival metaphysic of "spontaneous change" as presumptive as a teleological one. We are currently getting detailed deterministic explanations of the mutation of genes, but until something comparable is available to predict the generation of heterogeneous thought trials, this constitutes a weakness of the model.

3. The biological study of the evolution of any species takes the form of a post hoc reconstruction of a unique, "undetermined," historical process. The achievement of any general regularities must be probabilistic in the extreme. Studies starting from specific spectacular achievements in creative thought must be similar in nature.

4. The theory suffers from the multiplicity

[8] This paper does not attempt to review theories in this area. Citations to the important contributions of Pringle (1951) and Hebb (1949), do not begin to represent this literature. Note the special problem of a brain analogue to switching. Ashby's (1952) model and most computer memory search involves a spatial displacement of solids impossible in the brain, as in the stepping switch or the rotation of a magnetic memory drum. Computer memory search processes making use of timed pulses require a precision of timing dependent upon a stability of dimension presumably not available in the brain and usually if not always dependent upon a clock within which actual spatial displacement of solids takes place. It is for these reasons that Pringle's (1951) theory seems particularly promising, and one wonders why it has not been more used, or if not usable, more publicly refuted. See Pribram (1959) for a recent contribution to the problem of appropriate brain process models.

of possible mediations it posits. Where there are gaps in the historical knowledge, the theory makes available an embarrassing surfeit of possible reconstructions. In this, and in contrast to the successful theories of macrophysics, the theory is less self-disciplining, less specific in its predictions, more evasive of potential disconfirmation. This is perhaps the most important of Scriven's (1959) points, and one equally applicable to the theory of creative thinking.

5. In the usual applications, the environment is not described or describable prior to the organismic achievement of adaptation to that environment. Whereas the theory deals with an iterative process whereby an organism adapts to (achieves knowledge of) an independent environment, the evidences as to the organismic form and environmental parameters are often confounded, in that the same data series is used to infer both. While this is not so for laboratory studies of trial-and-error learning, it is particularly apt to be so for any study of truly great creative thinking in science. (See Campbell, 1959, p. 157 for epistemological citations to this problem.)

There is in addition, a serious problem which the blind-variation-and-selective-retention theory of creative thought faces which is not present in comparable degree in the modern theory of organic evolution. This is the unfavorable ratio of hypothesized unobservable processes to observable input-output variables. Note that even in its sketchy form here given, some 6 to 14 or more separately variable parameters are implied. These include: (*a*) A mnemonic representation of environment, varying perhaps in scope, accuracy, and fineness of detail; (*b*) A mnemonic search or thought-trial process, varying in the accuracy with which it represents potential overt exploration; (*c*) A thought-trial generating and changing process, varying in rate, heterogeneity, idiosyncrasy, and lack of repetitiousness among successive thought trials; (*d*) Selective criteria, varying in their number, accuracy of representation of environmental contingencies, and precision, sharpness, or selection ratio; (*e*) A preservation or propagation process, providing a retention for selected thought trials of a quite different order from the memory traces of the nonselected ones, varying perhaps in accuracy and accessibility; (*f*) A reality testing process in which the selected thought trials are checked out by overt locomotion in the external environment, varying perhaps in sensitivity to disconfirming feed-back.

This inventory of weaknesses does not, of course, represent argument in favor of rival theories of creative thought, which are judged to be still more amorphous, still less adequate. And as Duncan (1959) makes clear in reviewing the research literature on human problem solving, for all theories there is lacking a disciplined relation both to experimental undertakings and to findings. Even in its present form, however, the theory contains many empirical implications. Manipulation of any one of the 14 variables just listed should increase the number of creative products, providing the other variables can be held constant. Predictions of this order have been specified in the discussion of individual differences. Particularly characteristic are the unequivocal predictions regarding the volume and heterogeneity of thought trials. *Ceteris paribus,* a creative solution is more likely the longer a problem is worked upon, the more variable the thought trials, the more people working on the problem independently, the more heterogeneous these people, the less the time pressure, etc.

SUMMARY

This paper has attempted to make the psychological and epistemological point that all processes leading to expansions of knowledge involve a blind-variation-and-selective-retention process. Processes, such as vision and

thought, substituting for an overt trial and error are of couse acknowledged. But each of these are interpreted as containing in its very workability wisdom about the environment obtained originally by the blind variation of mutation and natural selection. In addition, each contains a blind-variation-and-selective-retention process at its own level. Supporting the effort to interpret all knowledge processes in this light has been an emphasis upon the tremendous gain in knowledge in the course of evolution and history, a gain which can only be explained by a continual breakout from the bounds of what was already known, a breakout for which blind variation provides the only mechanism available.

The application of this perspective to the process of creative thought antedates its application to trial-and-error learning in animals and to organic evolution, and is illustrated through quotations from Bain, Souriau, Mach, and Poincaré. The model is not in disagreement with the bulk of the Gestalt description of problem solving, nor the work on heuristically programed problem-solving computers. However, there is an effort to root out a prevailing implicit belief in the possibility of "direct" or "insightful" creative thought processes.

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